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Original Communications

PNEUMOPERITONEUM AND ROENTGENOLOGY AS AIDS TO MORE ACCURATE OBSTETRIC AND GYNECOLOGIC DIAGNOSIS*

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It is the purpose of this communication to give the results of some ten months' use of roentgenography after the injection of the pelvic and abdominal cavities with gas. Beginning the work with doubt and misgivings as to freedom from danger and practicality from the diagnostic standpoint, it can be emphatically stated that at the present time doubts have been cleared away and that it is one of the most useful procedures ever introduced into the clinic. Like all roentgenographic procedures, it requires long and close study before the plates of the pelvic and abdominal organs can be correctly interpreted. It was an easier matter after some experimenting to secure good plates from a technical standpoint, than it was to interpret what the plates showed.

Hence it follows that the obstetrician and gynecologist in order to obtain the quickest and most accurate returns from his work with pneumoperitoneum, must associate himself with an expert roentgenologist. Moreover, the latter must be interested in this new field and be ready to devote the necessary time to the interpretation of plates the like of which he may never have seen before. In fact it must be team work from the outset. To the gynecologist with his surgical expe-

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rience can be safely entrusted the extremely simple technic of gas inflation, either through the uterus or the abdominal wall. He can be instructed even in the methods of making the plates. It is asking too much, however, to expect the clinician at the outset to interpret what is depicted on the plates. His work and experience have not been along these lines, for he has had to employ the x-ray but seldom in his work.

Personally I am very fortunate in having associated with me in this pneumoperitoneal x-ray work my colleague, Doctor James G. Van Zwaluwenburg, without whose cooperation nothing could have been accomplished. Not only has he willingly spent many hours studying out the meaning of the various pelvic shadows, bringing into play his

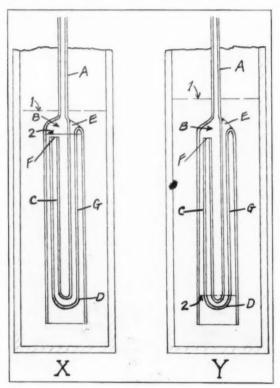


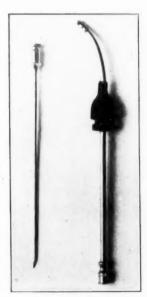
Fig. 1.-Syphon meter.

accurate and comprehensive knowledge of roentgenography, but his patience has been unfailing in guiding me along hitherto unknown paths.

When we began this work Doctor Van Zwaluwenburg and I agreed that elaborate, complicated, clinical technic must be avoided, if a method was to be developed available for the average diagnostician and clinic. Hence the gas inflation, practically from the beginning, has been made in the gynecologic examining room of the University Hospital.

Aside from the emptying of the lower bowel and bladder, the patient needs no special preparation for the gas inflation. In a few of our early cases when oxygen gas was employed, the patient was fortified by a hypodermic of morphine, but this is neither necessary nor expedient when the quickly absorbed carbon dioxide gas is used. Moreover, the use of morphine is inadvisable in the case of ambulatory patients who form quite a proportion of this material at the clinic, since out- as well as in-patients are inflated for diagnosis.

No expensive or elaborate table is necessary for gas inflation, merely one where a moderate tilting of the body downward can be secured. We have found this inclined position to be absolutely essential for the



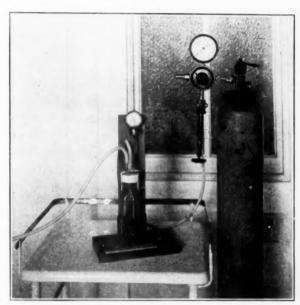


Fig. 2.

Fig. 3.

Fig. 2.—Needle and uterine cannula.

Fig. 3.—Carbon dioxide tank, gas pressure valve, syphon meter and manometer.

best results, since it is necessary for the pelvis to be freed from bowel coils if the pelvic structures are to be shown. This can be easily brought about by the use of the modified Trendelenburg posture with the patient in the prone position. One such table will suffice, although in the clinic it has been found more convenient to make the injection, either through the uterus or abdomen, on the ordinary examining table, transferring the patient to the other table for the x-ray of the pelvis.

At first we used the method of Stein and Stewart, employing a bag between the tank and the point of injection, forcing the gas through the needle by pressure on the bag. This method was abandoned in favor of Rubin's apparatus by means of which the gas can be passed under a pressure which is easily gauged, while the amount injected can be accurately measured.

The apparatus for measuring the gas is an inverted syphon pulsating meter used by the engineers for the measurement of chlorine gas. It is composed of an inverted glass syphon inside a cylindrical glass meter (Fig. 1). The operation of the meter is as follows: "When the downward flowing gas in A, reaches the point D, it will rush up through the tube G of the syphon and the bell or compartment B will refill with water up to the upper end of C. This completes one pulsation or measure of the meter and the amount of gas delivered by this one pulsation is, of course, the capacity of the compartment B between the points F and D." (Wallace and Tiernan Co.) With this apparatus connected with the gas tank it is only necessary to keep track of the number of pulsations to determine the amount of gas which is flowing through the cannula or needle provided there be no leakage. Any resistance to the gas flow can be readily determined by connecting the cylindrical glass-meter with a manometer. (Fig. 3.) For example, the manometer does not register when the gas is passing through the rubber tube connected with the needle with which the inflation is to be made through the abdominal wall. If, however, the tube be compressed at any point, the manometer index will rapidly rise according to the degree of compression of the tube and fall when the pressure on the tube is released. The same thing occurs when the flow of gas is impeded after the passage of the cannula into the uterus or the needle through the abdominal wall.

It is evident that before passing the gas into the abdomen or pelvis, it is necessary to regulate the amount of gas issuing from the tank under great pressure. We have had great difficulty in regulating the flow with the ordinary levers or wheels with which the tanks are provided. It really required more knowledge and experience to move the lever of the tank to secure the requisite gas flow than to use the cannula or needle. This difficulty has been done away with and the whole inflation made simple, easy and accurate by the use of a gas pressure reducing valve by means of which the flow of gas from the tank can be easily regulated. (Fig. 3.)

The uterine cannula is of metal of the Keyes-Ultzmann type perforated at the tip and provided with a rubber obturator for closure of the external os after the passage of the cannula so as to prevent the escape of the gas backward into the vagina. (Fig. 2.) The ordinary spinal puncture needle is well adapted for the perforation of the abdominal wall and the inflation of the abdominal cavity. (Fig. 2.)

The instruments necessary for the gas inflation are scrupulously sterilized before use. The abdominal wall or in ease the transuterine route be employed, the cervix, especially in the region of the external os, is prepared by the free use of iodine. Rubber gloves, sterile towels, etc., are employed just as in any surgical procedure. Unless these precautions be taken, it is easily conceivable that infection may follow gas inflation and not be due to any irritation set up by the gas itself. In order to settle the question whether oxygen or carbon dioxide gas as furnished in the ordinary tanks, contained bacteria, as has been claimed, numerous bacteriologic tests were made of these gases as they emerged from needle or cannula. In every instance the cultures showed no growths. It has been asserted that the low temperature of the gas as it issues under pressure from the tank gives rise to peritoneal irritation. This does not hold true for gas passing through the rubber tubes, because the temperature of this gas as it issues from the needle or cannula was always found to be that of the examining room.

Obviously one must use common sense in the selection of patients to be subjected to gas inflation. Up to the present time no patient with acute inflammatory pelvic disease has been inflated by either route. When the history of recent inflammatory disease is doubtful, even should the patient have a normal temperature, no chances are taken, but the inflation is conducted by the transperitoneal route. While neither shock nor depression have followed the gas inflation, it has been refrained from in certain individuals with impairment of the circulation. In other words the watchword of the clinic in this work has been to take no chances but to always be on the side of safety.

In none of the 300 cases of gas inflation has there been any sign of peritoneal irritation. Thus we think we are justified in the conclusion that with the proper selection of cases and with the proper technic there is no danger attached to gas inflation.

The transuterine is the method of choice since it throws much light upon the patency or nonpatency of the fallopian tubes. The gas can be introduced with as little discomfort to the patient by this as by the transperitoneal route. Where the tubes are found impermeable, the gas is introduced through the abdominal puncture. If the gas be passing freely through the tubes into the pelvic cavity, it is our custom to allow enough to flow so that a satisfactory x-ray of the pelvic can be obtained. In other words, the procedure of Rubin for determining the permeability of the tubes with small quantities of gas, has been carried a step further in case the tubes be patent. This has been done deliberately because of the impossibility with present methods of determining whether the gas be passing through one or both tubal openings. In case one tube be diseased and the other open, the condition of the pathologic tube will be shown by the pelvic x-ray.

The Sims has been found preferable to the lithotomy position where the gas is passed by the transuterine route. (Fig. 4.) The cervix is exposed after the introduction of the speculum and the anterior lip of the cervix seized by the tenaculum forceps after the cervix and adjacent portions of the vagina have been sterilized by iodine. Care is taken to remove all secretions from the cervical canal, a short distance from the external os. The general direction of the uterine canal is located by the sound and the uterine cannula passed, the rubber obturator being pressed within the canal until a snug fit is secured.



Fig. 4.—Method for transuterine gas inflation. Patient in Sims' position, perineum retracted, tenaculum forceps on anterior cervical lip, cannula in uterus.

Dependent upon the condition of the cervix, more or less outward pressure must be exerted upon the tenaculum, else the gas will escape backward into the vagina. It must not be forgotten to test the permeability of the cannula before its introduction. In case of blockage, the manometer index will quickly rise, showing that the instrument is faulty. Failure to observe this simple precaution for both cannula and needle may lead to wrong deductions.

The apparatus should have been previously set by means of the gas pressure valve for about six or eight pulsations a minute. Since the average gasometer of this type measures about 25 cubic centimeters of gas to each pulsation, the apparatus set at this rate of flow will allow of the passage of 125 to 200 cubic centimeters per minute. This amount should be used as a standard, as it has been determined by experience that this is about the maximum quantity which should be passed through normal tubes in one minute.

If the tube or tubes be permeable after the gas has been allowed to flow through the cannula there will be a rather rapid rise of the manometer to about 100 or 110. Then as the gas continues to flow there will be quite a sharp drop from 30 to 50 points, at which place the manometer index remains quite steady. On the other hand, if there be any resistance to the flow of gas through the tubes, there will be a steady rise of the manometer and the flow of gas should be stopped at the 200 point. At first we believed that if the gas did not pass at 150 after two or three trials it could be concluded that the tubes were closed. However, further experience, especially with patients under anesthesia, has shown that patent tubes may require the 200 pressure repeated four or five times before the gas will pass. Whether this means that some obstruction exists in these cases at the isthmic portion of the tube has not been determined.

At first we used too large quantities of gas, some two or three liters, with corresponding pain and discomfort to the patient. Experience has shown that this quantity of gas is unnecessary, if the patient's pelvis be raised and the table tilted so that the gas can rise in the pelvis. (Fig. 5.) This is accomplished after the proper amount of gas has been introduced by holding the patient on the tilting table face downward in the knee-chest position until an inclined board can be placed beneath the thighs. This board makes an angle of about 28 degrees with the plane of the table. Its upper portion is cut out and rounded so that the pubes come to lie just above the deepest portion of the notch. A plate changing tunnel is then placed horizontally on the table and the table tilted forward, the patient prevented from slipping by means of shoulder straps.

Experience has shown that to get the best results the table should be inclined at an angle of about 20 degrees. Excessive inclination of the table, when the pelvic organs are floated forward by the gas, causes too great displacement and makes the pelvic x-ray confusing.

A Coolidge portable unit operating on the ordinary lamp circuit furnishes the x-ray, the direction of the ray being in the long axis of the pelvis and perpendicular to the plate. (Fig. 6.) An 18-inch square of opaque fabric with a 6½ inch circular hole cut out of its center is laid on the patient's buttocks and serves as a diaphragm. Double screened films are used and the exposure varies from 14 to 20 seconds. The tube shift is in the long axis of the body and the stereo set so produced is used as though the patient were lying on the right side.

The best results so far as the x-ray plates are concerned, taking into consideration the comfort of the patient, are obtained by the use of from 800 to 1000 cubic centimeters of gas. Larger quantities of gas quickly injected cause great discomfort and even pain to the patient.

Hence it has been our constant aim to reduce the amount of gas injected to a minimum, our efforts being greatly aided by the use of the table arrangement just described. Since the patients complain of a sense of fullness in the lower abdomen and some discomfort when only 300 to 400 cubic centimeters of gas have been introduced and since that quantity has been found too small for proper inflation, it is obvious that a certain amount of discomfort will always be present. However, the sensation, excluding highly nervous women, may be described as discomfort and not the pain which always accompanies overinflation.

At first we used oxygen gas but abandoned it because it was slowly absorbed with corresponding persistence of the discomfort. Deflation

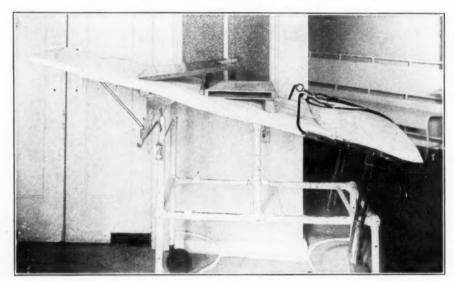


Fig. 5.—Table for taking pelykogram, showing notched inclined plane, tunneled plate holder and table at proper inclination (20°).

relieved to some degree the symptoms but necessitated a second needle puncture, which for obvious reasons did not improve the technic. For these reasons we substituted for the oxygen, carbon dioxide gas which is absorbed so far as symptoms of discomfort are concerned in from twenty minutes to one-half hour. After inflation with this gas patients return to the ward or leave the hospital without discomfort a half hour after the plates have been taken. In fact the one disadvantage of this gas is the necessity of having the technic of plate taking work perfectly smoothly. Delay beyond a certain point means so much carbon dioxide gas absorption as to materially lessen the chances of good roentgenograms.

In addition to acute pelvic disease, profuse purulent and bloody uterine discharges are considered contraindications to the passage of the gas by the transuterine route. In such cases the gas is passed through the abdominal wall by the following technic: The skin of the abdominal wall from the umbilicus nearly to the pubes and for a width of eight inches is sterilized with iodine. A fold of the abdominal wall just below the navel is compressed laterally with the left hand and raised upward and made tense. A spinal puncture needle is passed through this fold in a slightly upward direction at a spot in the median line one and one-half inches below the umbilicus. (Fig. 7.) If the skin be made tense and the needle be sharp, no preliminary anesthesia of the skin will be necessary. The needle is passed downward

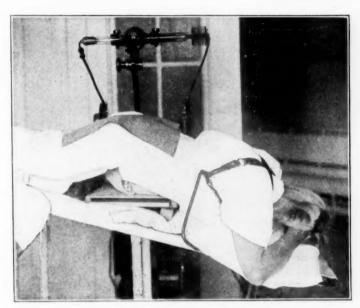


Fig. 6.—Patient after inflation in prone (partial knee-chest) position on inclined table. Opaque diaphragm, Coolidge portable unit arranged so that ray will fall perpendicular to pelvis and plate.

until it meets the resistance of the fascia, the left hand still keeping tense the fold of the abdominal wall. The needle is then thrust through the fascia and peritoneum, a little experience giving accurate information to the hand when the peritoneum has been pierced. The stilet is then withdrawn from the needle and the latter connected with the rubber tube leading from the syphon meter. The latter is set by means of the pressure valve to about ten pulsations to the minute, so that it will require approximately four minutes at the rate of 25 cubic centimeters of gas at each pulsation for 1000 cubic centimeters of gas to pass into the peritoneal cavity.

The objection that in case the needle does not pierce the peritoneum, the gas may be forced through the subcutaneous tissues is not valid, since by the manometer it can be told at once whether the gas is flowing freely into the cavity or is meeting with an obstruction. When the needle has penetrated the peritoneum the manometer index rises first some 15 or 20 points but falls usually immediately to almost zero. In case this fall does not occur the patient should be directed to take a deep breath which will have the effect of lifting the point of the needle from anything which may be obstructing it.

At first the skin at the point of puncture was anesthetized by Schleich's solution but this has been discontinued since it was found that very nearly as much pain was produced by this procedure as by the passage of the needle. Obviously if the point of election for the puncture is over what may be an adherent intestine or if a neoplasm



Fig. 7.-Transperitoneal gas inflation. Needle in abdominal cavity and gas flowing.

lies beneath this site, another portion of the abdominal wall must be chosen.

Before the patient is inflated in addition to a careful history of her case she is subjected to a most complete bimanual pelvic examination to which is added a rectovaginal examination if the case seems to call for it. Without going into details it will suffice to say that an attempt is made in every case to record on the examination sheet what the pelvis contains, not in general but in specific terms. For example, if in the case of inflammatory disease to the left of the uterus the ovary and the tube can be made out, it is so stated. If neither can be recognized by the examining finger it is so recorded. The same holds true regarding adhesions, relation of neoplasms to the uterus, etc. In other words such terms as sensitive mass, boggy mass, left-sided inflammatory have been disearded since our work has been checked up

by the x-ray. We may and do make mistakes but at least they are definite errors from which valuable lessons can be learned.

The plates are taken to the x-ray laboratory and interpreted by Doctor Van Zwaluwenburg without reference to the clinical findings. Then the clinician and roentgenologist go over the plates set up in stereo in an endeavor to reconcile the two findings. If the case be operative, the patient is carefully examined under anesthesia and the

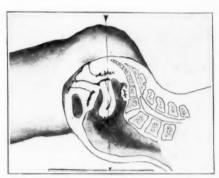


Fig. 8.—Correct inclination of patient, x-ray in axis of pelvis and perpendicular to plate. In this position with movable uterus the anterior and posterior culdesacs will be filled with gas. Plate will show cross sections of isthmus and fundus.

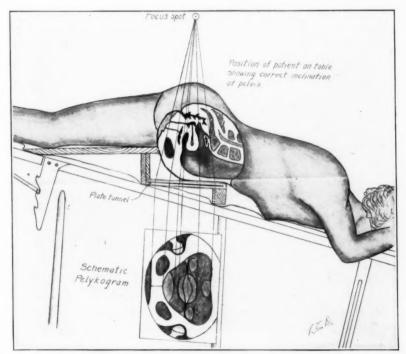


Fig. 9.—Correct inclination of pelvis with schematic pelykogram showing theoretical projection of pelvic organs. Partial knee-chest position secured by prone position of patient on frame and a 20° inclination of the table.

actual findings, uninfluenced by the x-ray study of the plates, are recorded. If the abdomen be opened, the position and condition of the pelvic organs are dictated at the time and the final records of the case together with any removed organs are studied with the plates.

It certainly has been surprising to see how often it has been impossible to determine accurately by the examining finger the exact condition of the pelvic organs. I had failed to realize how much I was depending upon the opening of the abdomen to clear up fine points in diagnosis. From a study of the history together with a thorough pelvic examination it was decided whether the case was an operative one or not. The patients were always examined under anesthesia prior to operation but not so much to determine more accurately the exact pelvic pathology as to confirm the opinion that the case was one for operation. For always the easier method of making the exact diagnosis was ahead of one since everything was to be cleared up after the incision had been made.

All this has been changed since the x-ray of the pelvis has been utilized as an aid to diagnosis. The clinician is placed upon his mettle, so to speak. He carefully weighs his opinion regarding the size and position of the uterus, the tubes and ovaries, if he is to go over his recorded findings and compare them with those of his colleague whose only knowledge of the case is what he sees depicted on the roentgenograms. Renewed interest is aroused in being able to diagnosticate prior to operation what had been considered rather minor pelvic conditions, such as the exact position of the ovaries and tubes, or whether they be free or slightly adherent. At any rate it is a fact that preoperative diagnosis, since the advent of the pelvic x-ray, has received a tremendous impetus in the clinic and has greatly improved.

In definitely stating what can be seen in technically good roentgenograms of the pelvis it must be borne in mind that this new field is constantly changing. What could not be seen at the beginning of our work is perfectly apparent today and the same will probably apply to interpretations of plates months from now. However, enough can be made out at the present time to be of great aid in diagnosis.

The pelvic organs are represented on the plate by optical cross sections at the point of tangency of the projecting rays. (Figs. 8 to 20.) The uterus is nearly always clearly outlined and one can judge of its position, size and contour. If not drawn to one side by adhesions or displaced by a neoplasm, so that the ray as it is shot in the axis of the pelvis catches it fairly, the uterus will show two cross sections, one of the body and one of the isthmus or supracervical portion. It has been found that the isthmus is greatly enlarged and extends more into the broad ligaments in cases of pregnancy than in the nonpregnant. So striking is the change in the isthmus that by this sign alone it has

been possible to diagnosticate pregnancy by the x-ray as early as the sixth week and before softening can be determined definitely by the examining finger. The sign is constant from two and one-half months onward, but we have not had sufficient material to date to be too dog-

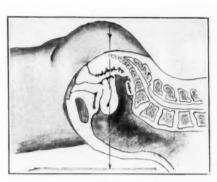


Fig 10.

Fig. 11

Fig. 10.—Insufficient inclination—no gas in vesicouterine pouch. Failure to differentiate anterior surface of uterus—may or may not project isthmus.

Fig. 11.—Excessive inclination. Restriction of posterior culdesac with failure to differentiate posterior surface of uterus and appendages.

matic about the presence of this enlargement of the isthmus in every case of early pregnancy.

Irregularities of the uterus produced by fibroids and especially calcification changes in these growths are readily recognized on the

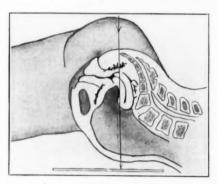


Fig. 12.

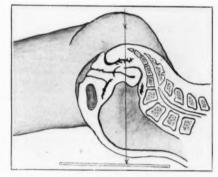


Fig. 13.

Fig. 12.—Retrocession. Obliteration of posterior culdesac and excessive width of the anterior culdesac. Simulates excessive inclination of pelvis.

Fig. 13.—Adherent retroversion. Complete failure to differentiate uterus. Often complicated by adherent bowel shadows.

x-ray plate. Through the changes in the isthmus described above it has been found possible to diagnosticate positively the occurrence of pregnancy in a fibroid uterus at a stage when it could not be considered probable from the history and clinical findings. In a num-

ber of cases where the patient has been exposed to pregnancy and where menstruation is irregular it has been possible to state definitely that the patient was not pregnant because of the size of the uterus and the absence of change in the isthmus.

Since experience has determined just the right inclination of the

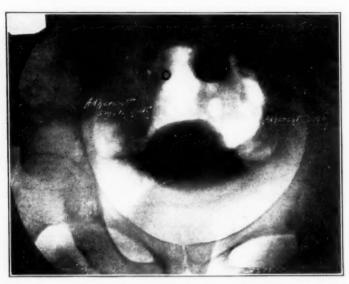


Fig. 14.—32925—Uterus sligh ly enlarged in the axis of the pelvis, appendages lost in the thickening on the inferior lateral wall of the pelvis and obviously adherent. An example of the spontaneous replacement of a non-adherent retroverted uterus.

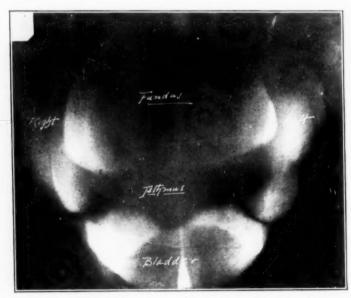


Fig. 15.—30426—Three months' pregnancy. The entire upper portion of the pelvis is filled with a rounded shadow representing the fundus of the uterus. Broad expansion of the isthmus, conspicuous round ligaments placed on the stretch, appendages hidden under the shadow of the fundus.

patient in the prone position on the table to bring about the proper forward and downward displacement of the appendages, normal ovaties and tubes can be shown on the x-ray plates with great regularity. The size of the ovary can be quite accurately estimated and usually it is perfectly possible to differentiate between the free and the adherent organ.

It is not so easy to see normal tubes unless the gas has been passed by the transuterine route when from their distention they are quite clearly made out. Enlarged nodular noncystic tubes are well depicted upon the plates and not infrequently are much better seen than

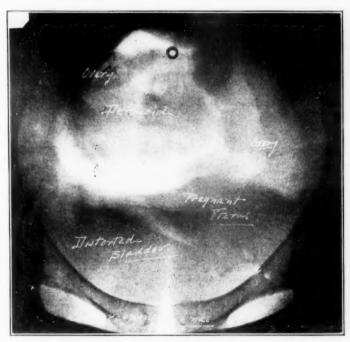


Fig. 16.—32024—Dermoid cysts complicated with six weeks' pregnancy. The uterus is slightly enlarged with conspicuous and enlarged isthmus displaced to the left by the large rounded shadow of the dermoid occupying the right posterior portion of the pelvis. The right ovary is displaced upwards and to the right. The left ovary is somewhat enlarged, the bladder partly distended and deformed by pressure.

palpated. Especially is this true when the tubes are adherent to the sides of the pelvis and covered with plastic exudate. In these cases the clinician has long realized the difficulties of palpation and has depended upon the history, thickening in the region of the broad ligaments and decreased uterine movability as diagnostic signs of the condition.

Pus tubes with adhesions to the broad ligaments and bowel coils are particularly well seen on the x-ray plates. To be sure the clinician usually has little difficulty in palpating fairly distinct sacular distention of the tubes but when the pelvis is the seat of an extensive inflammatory process it is often quite impossible to differentiate the distended tubes. From experience the examiner judges what will be found when the abdomen or posterior culdesac is opened but his differentiation of the contents of the pelvis is not nearly as definite as can be brought out by the x-ray.

Pelvic neoplasms if not so tightly wedged in the pelvis as to prevent the gas from surrounding them are clearly shown by the roentgenograms. Since it is perfectly possible to show the outlines of the liver, spleen and kidneys by pneumoperitoneal x-ray plates of these



Fig. 17.—5.4.59—Fibroid of the uterus and bilateral adherent ovaries. The irregularly enlarged uterus occupies the axis of the pelvis, both ovarian shadows are small, lying on the floor of the pelvis and at a lower level than the fundus. On the left there is some evidence of irregular adhesions. The position of the ovaries on the floor of the pelvis in the modified knee-chest position is indicative of their fixation by adhesions.

organs, usually it can be determined by a study of roentgenograms of the pelvis and upper abdomen whether neoplasms arise from the pelvis or elsewhere and this means of diagnosis entails far less pain or discomfort than catheterization of the ureters or other diagnostic measures.

The above will suffice to show the possibilities of the pneumoperitoneal x-ray, since detailed clinical and roentgenographic findings will be dealt with in a subsequent paper. While pointing out what has been and can be accomplished by the new diagnostic procedure, it must not be thought that the idea is to belittle or do away with bimanual pelvic examination. In quite a proportion of cases information derived from this source proved far more accurate than that furnished by the x-ray. This was to be expected for bimanual examination has been practiced for years, while this kind of x-ray work is new and confined to only a few hundred observations. In fact nothing is to be



Fig. 18.—32702—Large fibroma occupying the upper portion of the pelvis and showing a nodular shadow representing calcification of a fibromatous nodule. An enlarged tube is seen presenting from behind the mass in the right posterior portion of the pelvis.

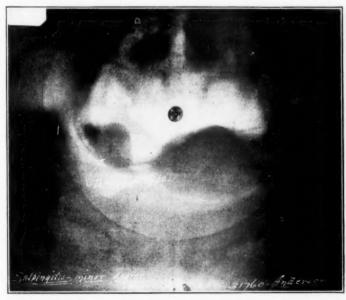


Fig. 19.—31760—Salpingitis, minor degree. Insufficient inclination of the patient, producing a narrow anterior culdesac. The right tube can be distinctly recognized as a thickened cord. Otherwise negative.

gained, on the contrary much will be lost, by raising the issue of antagonism between the two diagnostic methods. Just as it is scarcely fair to demand that the roentgenologist interpret the pelvic x-ray plate without any knowledge of the case, so it is asking rather too much of the clinician in certain cases to state definitely the condition of the pelvic organs from bimanual examination alone. The rule of making separate diagnoses has been followed by us deliberately in order to gain the most knowledge from our different diagnostic methods.



Fig. 20.—33221—Bilateral salpingitis. The fundus of the uterus is approximately in the normal position, flanked on either side by appendages of increased size and density, more marked on the right. The included gas shadows in the right side prove the presence of adherent bowel coils over the appendage.

ods. From now on it is planned to give due consideration to the other's findings in order that the most accurate diagnosis can be arrived at by the fullest cooperation.

It must be borne in mind that only about 300 patients have been subjected to the pneumoperitoneal x-ray. This means that the work, compared with tens of thousands of gastrointestinal x-rays, is just beginning. Greater study and experience will lead to such accuracy of diagnosis that it will be considered as faulty in doubtful cases to do pelvie work without a pneumoperitoneal x-ray, as to operate upon the stomach or upper abdomen without a roentgenogram.

Since it cannot be expected of the roentgenologist that he make clinical examinations, probably perfected diagnostic technic will come from the clinician who has made himself master of the pneumoperitoneal pelvic roentgenogram. With his knowledge of pelvic pathology which comes from his everyday work, he is better equipped than any one to interpret the pelvic plate provided he is willing to give up the necessary time and study to its interpretation. At least this is not too much to hope in the case of the young obstetrician and gynecologist just starting on his special work. For I am convinced that if we are to demonstrate that there is real need for our specialty, it must be through developing preoperative diagnosis. It should be the aim of the gynecologist by accurate diagnosis to state with ever greater and greater positiveness when an operation is indicated and when the symptoms can be made to disappear by other methods of treatment.

SUMMARY AND CONCLUSIONS

- 1. The pneumoperitoneal x-ray is a great aid to accurate obstetric and gynecologic diagnosis.
- 2. In suitable cases and with the proper technic, gas inflation is free from danger.
- 3. The apparatus for gas inflation and pelvic roentgenography is simple and inexpensive and can be used in any obstetric or gynecologic examining room.
- 4. The method should not be used in cases of acute pelvic inflammation or when disturbances of circulation may arise from sudden abdominal distention.
- 5. Since carbon dioxide gas is absorbed within half an hour, it is preferable to oxygen gas for inflation, since the latter gas may not be absorbed for days.
- 6. Whenever possible the transuterine should be chosen in preference to the transperitoneal route for the introduction of the gas because of the valuable information it furnishes regarding the permeability of the fallopian tubes.
- 7. Excessive quantities of gas cause great pain. Experience has shown that for the ordinary case 1000 cubic centimeters of gas will cause only moderate discomfort and are sufficient for good roentgenograms.
- 8. With the proper position (partial knee-chest with tilted table) and the ray directed perpendicularly to the plate in the axis of the pelvis the pelvie organs are clearly shown by roentgenography.
- 9. Experience with pneumoperitoneal pelvic roentgenography will enable the observer to diagnosticate with great accuracy the pathologic changes in the pelvic organs.
- 10. There is every indication that pregnancy can be diagnosticated as early as the sixth week by the pneumoperitoneal x-ray.
- 11. Bimanual pelvic examination and pelvic pneumoperitoneal roentgenography are not antagonistic diagnostic methods. Each is valuable and their value is enhanced if they be used in conjunction, each acting as a check upon the other.

(For discussion, see page 433.)

VAGINAL SUPRACERVICAL HYSTERECTOMY WITH INTER-POSITION OF THE CERVICAL STUMP FOR CYSTOCELE AND PROCIDENTIA ASSOCIATED WITH EN-LARGEMENT OF THE UTERUS*

BY HIRAM N. VINEBERG, M.D., F.A.C.S., NEW YORK CITY

N FEBRUARY, 1915, I presented a short communication to the Obstetric Section of the New York Academy of Medicine upon the operation designated in the title of this paper. I drew attention to the circumstance that not infrequently in procidentia and cystocele, the uterus was found too large to be suitably interposed. The enlargement most often was due to what has been variously termed as chronic metritis, fibrosis, areolar hyperplasia and more recently simple hypertrophy. The enlargement might also be due to fibroid nodules of various size or to a single fibroid growth which increased the volume of the uterus to the size of the gravid organ at the eighth week. With any greater enlargement of the uterus it is questionable whether the vaginal operation would not be found too difficult and tedious, although I have performed it in a few instances where the uterus was the size of the gravid organ at the tenth and twelfth week. Again there are cases when the uterus may not be increased in size and still not be suitable for interposition, e.g., cases of persistent bleeding, the so-called idiopathic metrorrhagia. The surgical procedure hitherto employed to reduce the size of the uterus in the above class of cases has consisted in the excision of a wedge-shaped piece either from the anterior wall or from the fundal area. Most operators, among whom I number myself, found the method unsatisfactory because it was rare to get primary union of the very much thickened and certainly not normal uterine walls. Protracted suppuration was almost the rule and the mortality was rather high.

Another procedure resorted to in these cases is a total hysterectomy with suturing of the round ligaments (Goffe) or the broad ligaments (Mayo). This procedure, however, is usually restricted to elderly women with very much stretched and thinned out tissues.

A third procedure and the one forming the basis of this paper consists in amputating the body of the uterus at or above the level of the internal os and fixing the cervical stump to the subpubic ligament, thus forming a firm and solid support for the bladder. When, as fre-

^{*}Read at the Forty-sixth Annual Meeting of the American Gynecological Society, Swamp-scott, Mass., June 2 to 4, 1921.

quently obtains, the vaginal portion is very much elongated or lacerated and the lips everted and hypertrophied, it is amputated and there will still remain a good-sized stump for the purpose of interposition. The redundant anterior vaginal flaps are excised and sutured to the cervical stump as is done in the usual interposition operation.

As far as I could ascertain from a search of the literature, Löwit (Zentralbl. für Gynäk., 1911, xxxv, 297) was the first to employ the cervical stump after amputating the body of the uterus as a *pelotte* to hold up the bladder in cases of procidentia and cystocele. The technic I have developed differs from that of Löwit in two particulars: (1)

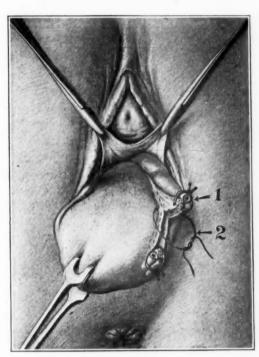


Fig. 1.—Shows uterus everted, left adnexa ligated and removed (1); ligature thrown around the uterine artery but not tied (2).

the application of the subpubic suture (Fig. 3) which I consider a great advantage, (2) in the amputation of the vaginal portion when it is markedly elongated or diseased.

The steps of the operation are as follows: (1) Longitudinal incision of the anterior vaginal wall extending from near the urethral meatus to within an inch or an inch and a half of the cervical os. (2) Separation by sharp or blunt dissection of the vaginal flaps from the underlying bladder. (3) The bladder is pushed up from the uterus and the base of the broad ligaments by gauze or scissor dissection. (4) Transverse incision of the vesico-uterine fold. (5) Delivery of the

body of the uterus through the vaginal incision. Up to this the steps are identical with those of interposition of the uterus. (6) If the ovaries are to be retained a ligature is passed around the ovarian ligament and the uterine end of the tube on either side and the tissue cut between the ligature and the uterus (Fig. 2). If the ovaries are to be removed for one reason or another the ligature is passed around the infundibulopelvic ligament containing the ovarian vessels. (7) A ligature is passed at the level of the internal os on either side to embrace the uterine artery (Fig. 1). The body of the uterus is then amputated at the desired level by a curved incision with the concavity

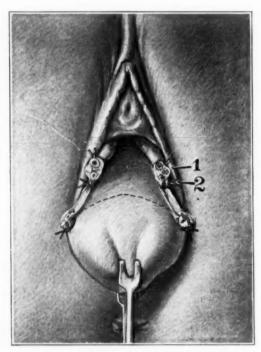


Fig. 2.—Shows everted uterus with both broad ligaments ligatured and cut. Dotted line indicates level of amputation of the body of the uterus.

towards the cervix (Fig. 2). The edges of the wound on the cervical stump are carefully coaptated by chromic catgut sutures. When the patient is under forty years of age and it is desirable to retain the menstrual function, the incision should be made higher up on the body so that a portion of the body with the corporeal endometrium is left with the cervical segment. This was done in a few of the cases and menstruation, though scanty, was preserved. (8) The cervical stump is now fixed to the subpubic ligament in the following manner: A chromic catgut suture is carried laterally for the distance of about an inch through the subpubic fascia underneath the vaginal mucosa then pene-

trates the cervical stump from behind forwards (in other words the suture transfixes it) and is carried through the subpubic fascia in the opposite direction from the other side (suture No. 1 in Fig. 3). Note that the vesicouterine peritoneum is purposely avoided in this suture. When the suture is tied, the cervical stump is brought up tightly against the raw tissues underneath the pubic arch and a firm union must result. With such a solid plug fixed to the subpubic ligament it is impossible for the bladder to come down. This assumption has been confirmed by the subsequent observation of the patients operated upon. If the vaginal portion is eroded, lacerated, and hypertrophied

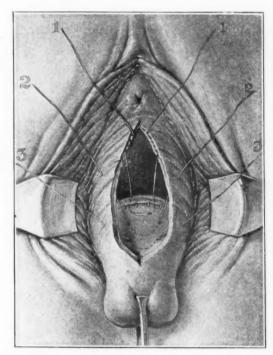


Fig. 3.—Shows manner of introducing sutures: (1) passing through uterus and subpubic ligament; (2 and 3) vaginal sutures including cervix,

or considerably elongated, it is amputated in a suitable manner and the resulting wound carefully coaptated by chromic catgut sutures. Should the vaginal flaps be redundant, a strip of the proper width is excised from either one. They are then sutured together, the sutures being carried through the anterior wall of the cervical stump. In some instances the anterior vaginal wall being longer than the cervical stump a transverse fold forms on either side. The edges of these folds can be sutured together by a running catgut suture. The operation is completed by doing the necessary plastic work upon the posterior walls and perineum. It has occurred to me that perhaps an additional

support to the bladder might be obtained by suturing together the broad ligaments as in the Mayo operation, but the observation of my results has convinced me that any additional support to the bladder is not needed. Therefore there would be no sense in adding to the length of the operation, which is time consuming as it is, by any unnecessary elaboration.

I began doing the operation in 1912 and until the time of my presentation in February, 1915, I had performed it in 10 cases. From that date until March 1st, 1921, on the second Gynecological Service of Mt. Sinai Hospital, of which I had charge, the operation had been performed in 53 cases; of these 27 were operated upon by myself, the remaining 26 by my associates on the service and the different house surgeons. During this period I have performed the operation in 20 private cases, making a total of 83 cases. It is interesting to note in what proportion of cases of procidentia and cystocele the uterus was found too large for interposition. I deem the uterus as being too large when it is the size of the gravid organ at the fifth or sixth week, or when the walls are very thick and hard. In the period covered by the series of cases reported in this paper I had in private and public practice 226 cases of interposition for procidentia and cystocele, consequently in 309 successive cases of procidentia and cystocele there were 83 cases, or 37.2 per cent, in which the uterus was found too large to be interposed. There was one death in my own personal series of 57 cases, i.e., a mortality rate of 1.8 per cent. There was one death in the 26 cases on the service operated upon by others than myself. The mortality, therefore, for the combined series of 83 cases, was 2.4 per cent. In my own fatal case the patient had been doing well for 12 days, she then developed some fever and on opening the upper angle of the vaginal wound a small amount of pus and tarry blood escaped. A small gauze drain was inserted. The fever then subsided but four days later, during my absence symptoms of intestinal obstruction set in. An exploratory abdominal section was done and purulent peritonitis was found. The patient died the next day. Suppuration in the bed of the interposed cervix occurred in 6 cases. In opening up the upper angle of the wound the process rapidly disappeared with the exception of two cases which took two and three weeks, respectively, to clear up. The final anatomic result was not impaired by the complication.

With increased experience in the technic of the operation the cases of suppuration have been decreasing. It is important to trim the pedicles closely and to effect a good coaptation of the edges of the cervical stump. As nearly all of these cases are associated with marked congestion of the tissues and engorgement of the blood vessels I usually insert a small gauze drain between the vaginal flaps and

the side of the cervical stump. This is removed in 48 hours. Taking into consideration the local conditions in many of the cases at time of operation, the extensive erosions of the cervix, with distended and engorged infected nabothian follicles, with marked edema of the vaginal walls, it is surprising there were comparatively so few cases in which suppuration occurred. Many of the cases should have been subjected to a fairly long course of preparatory local treatment. But in the stress of present-day life, it is difficult to carry this out in private practice, at least in New York City. It is not any easier in public practice as it is not possible with a limited number of ward beds to keep a patient in the hospital two or three weeks prior to operation. In two cases, vesicovaginal fistulæ followed the operation in the hands of my associates. These closed spontaneously after an interval of two or three weeks.

30-35	35-40	40-45	45-50	50-55	55-60	60-65
2	16	29	23	8	3	2

The ages varied from thirty to sixty-five years. Sixty cases, or 72.3 per cent, occurred between the ages of forty and sixty-five years.

ULTIMATE RESULTS OF THE OPERATION

In the private series of 30 eases we have to deduct the 6 cases operated upon during the past twelve months as being too recent upon which to report. The remaining 24 cases have been under observation from one to nine years. There was one failure. There was a recurrence of the prolapse of the vaginal walls and the cervical stump prolapsed to the introitus. The parts, however, have been easily kept up by a small ring pessary which was not possible to do prior to the operation. This was among the first five cases operated. One patient continued to complain of a great variety of pelvic symptoms although the anatomic result was good. In the remaining 22 cases the anatomic results are good in all and in a great number of them they are simply ideal. The stump of the cervix is high up in the vault of the vagina, it is movable and there is no protrusion of the bladder, even when the patient bears down. All of the patients are free from all pelvic symptoms.

The data of the ultimate results of the hospital ward series of 53 cases are very meager, inasmuch as the follow-up clinic in the hospital has as yet not been fully developed. The patients were requested to return periodically for observation, but only 13 of them followed the request. In one of these cases there was a recurrence of the cystocele and in a second case there was slight protrusion of the anterior vaginal wall. In a third case the vaginal portion which had been very

much elongated but which had not been amputated, protruded outside the introitus a few months after the operation. I amputated the vaginal portion afterwards and the result since then has been most satisfactory. In the remaining ten cases the results were perfect.

My experience with our follow-up system as it is constituted, has taught me that in most conditions it serves no guide as to actual results, for as a rule only those patients return who have something to complain of, no matter how remote a bearing it may have to the operation. Hence it may be fair to assume that of the 40 patients who did not return the results were good in the majority of them.

CONCLUSIONS

- (1) In over one-third of the cases of procidentia and cystocele the uterus is found too large to be adapted for interposition.
- (2) Decreasing the size of the uterus by excising a portion of the anterior wall or fundus has been found unsatisfactory on account of the rarity of obtaining primary union of the thickened and diseased walls of the uterus and the consequent high morbidity and mortality.
- (3) A much more advantageous procedure consists in amputating the body of the uterus at the level of the internal os or higher up (if the patient be under forty years of age) and interposing the cervical stump.

751 MADISON AVENUE.

(For discussion, see page 424.)

THE INTERPRETATION OF VESICAL SYMPTOMS IN GYNECOLOGIC DIAGNOSIS*

By F. E. KEENE, M.D., PHILADELPHIA, PA.

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S YMPTOMS of deranged bladder function play an important rôle in the histories of many gynecologic cases and the proper interpretation of these symptoms presents problems of intense interest from the standpoint of diagnosis, and not infrequently it furnishes data upon which depend the indications for appropriate remedial measures. For a number of years I have been especially interested in the investigation of such symptoms and have made routine cystoscopic examinations in all patients presenting vesical symptoms irrespective of the nature or extent of the pelvic pathology. In the majority of cases the alterations in the bladder have been of a purely secondary nature, but their visual demonstration has proved an attractive study; on the other hand, we have been able to discover associated lesions which were entirely independent of the pelvic pathology, lesions often of more vital import than those within the pelvis whose presence might have remained unsuspected had symptoms alone formed the basis of diagnosis. Again, cases have been referred for the correction of some minor gynecologic anomaly supposed to be responsible for the vesical symptoms which, in reality, played no part whatever in their production, the actual lesion being in the bladder itself or upper urinary tract.

This paper will include a brief résumé of our findings, taking up first the alterations in the bladder, ureters or kidneys which are direct sequelae of the pelvic pathology, and secondly, certain types of associated lesions which we have encountered in our investigations. The object of this presentation is to emphasize the importance of detailed cystoscopic studies in all patients with vesical symptoms rather than the dependence upon the clinical history or laboratory examinations for their interpretation.

Because of its intimate anatomic relationship with adjacent structures, the bladder often participates in the pathology of these organs, whether this be in the form of a displacement, neoplasm or infection. The vesical symptoms arising from such participation naturally vary in degree as well as kind, and depend primarily upon the nature of the pelvic lesion and secondarily upon the subsequent changes either

^{*}Read at the Forty-sixth Annual Meeting of the American Gynecological Society, Swamp-scott, Mass., June 2 to 4, 1921.

within the bladder alone or in combination with the kidneys and ureters which may in their turn become involved.

Distortion of the bladder incident to the direct pressure or traction of benign tumors deserves but passing mention. Here the symptoms are primarily of a purely mechanical origin and are characterized by frequency of urination, especially just prior to and during menstruation, or more rarely by partial or complete obstruction to the passage of urine. Painful urination is usually the manifestation of an infection which may, with ease, be engrafted upon a chronic retention. Hematuria is rarely found in association with uncomplicated benign tumors and, when present, may be due to small varices in the bladder wall or to minute papillary excrescences of the mucosa in the immediate vicinity of the tumor, both incident to impaired circulation. Consequently, hematuria in association with a benign pelvic tumor should never be considered a secondary manifestation until proved so cystoscopically, for it usually signifies a lesion in the bladder or kidney entirely independent of the pelvic lesion. Likewise pyuria, in the absence of an antecedent history of retention or evidences of an infected tumor, is an uncommon secondary event and its presence should be the indication for cystoscopic study to determine the exact nature and location of the infection.

Malignant tumors, especially carcinoma of the cervix, frequently produce vesical symptoms due to direct extension of the growth to the bladder or secondary to renal infection incident to incomplete occlusion of the ureters. Passing over the extensive bladder changes and symptoms common to advanced cases of cervical cancer, it is worthy of note to emphasize the value of cystoscopic study in the presumably early cases, for by this means one may find data of value in determining operability. This applies particularly to the cervicofundal type of growth which to external appearances offers a favorable operative result; on cystoscopic examination, however, one may find changes in the bladder wall indicating an extension of the growth beyond the confines of the uterus, in which operation would be a futile procedure.

The majority of cases of acute pelvic infection, with associated vesical symptoms, present little or no difficulty of interpretation, due to extension by continuity along the urethra or to direct contact between the bladder base and the diseased structures; here, eystoscopic studies are not only unnecessary but often inadvisable. In the chronic cases, however, the presence of persistent bladder irritability is certainly an indication for such investigation to determine the nature and extent of the disease, whether it has arisen as a purely secondary manifestation or is a coincident lesion, independent of the primary pelvice

infection and to what degree, if any, the ureters and kidneys are sharing in the production of symptoms.

Passing over without comment the ordinary evidences of chronic urethral and bladder infection, we shall consider briefly a few observations relative to an associated ureteral and renal involvement.

In their course along the lateral pelvic walls, but especially through the broad ligaments, the ureters are brought into such intimate contact with infections of the uterus and its adnexa that their participation in the disease would seem a most likely as well as a common occurrence, arising either from direct extension of the process in cases of acute infection or from compression incident to contraction of a pelvic exudate in the chronic cases. Our experience has been, however, that while such potentialities are present, their actual occurrence is not common, for in only a few instances have we been able to demonstrate a stricture of the ureter or a pyelitis which we could look upon as the result of an associated pelvic infection.

The diagnosis of a secondary pyelitis is not warranted unless definitely established by eareful cystoscopic studies, for occasionally an acute pelvic infection may present the symptoms of renal involvement, and yet such does not exist. In five of these cases we have found the ureters and kidneys normal both anatomically and functionally, with sterile cultures of the urine. One case with a large pelvic abscess subsequently developed a perinephritic abscess; in the others, the renal symptoms rapidly disappeared with the subsidence of the pelvic infection. Here we were unquestionably dealing with a perinephritis and not a true renal infection arising from the primary focus in the pelvis.

Hunner has called attention to the frequency of ureteral strictures which may give rise to vesical and renal symptoms and which occur secondary to infections within the pelvis or in more distant structures. While we are not in full accord with his views on the subject, we are of the opinion that he has opened up an interesting field of investigation, and that in obscure cases of protracted vesical irritability and pain, this lesion must always be considered and appropriate measures used to satisfactorily eliminate its presence.

Our studies have been made in the types of infection commonly seen in a gynecologic service and do not include in any great number the acute puerperal infections which, from the nature of their pathology, offer ideal conditions for the development of renal complications and in which such complications are often encountered.

In cases of extensive prolapsus and cystocele, the symptomatic manifestations of impaired bladder function are primarily mechanical in their origin, but later may be modified by the addition of an infection, the invasion of which is made easy by the distorted anatomy of the urethra and bladder. The interpretation of the symptoms which these

patients give, such as incontinence, frequency, retention, etc., is readily demonstrated by cystoscopic examination, and the findings are more or less uniform, differing only in degree. The urethra shares with the bladder base in its loss of support and because of the sagging of its posterior segment, the canal assumes a direction more vertical than horizontal; there is also dilatation of the posterior urethra with an attenuated, edematous sphincter, irregular in outline and showing pseudopolypi projecting from its edge. There is not only a sacculation of the bladder base but an actual descensus of the trigone along the anterior cervical wall carrying the ureteral orifices to a lower level than normal. The mucosa of the trigone is edematous and its blood vessels are deeply engorged. Thickening of the muscularis is evidenced by widespread trabecular hypertrophy. Some degree of retention is common in large cystoceles and this stagnation invites infection which modifies the picture by the changes characteristic of cystitis.

The study of these cases is not complete until one has determined to what extent, if any, the ureters and kidneys have become affected by the distortion of the bladder. The descensus of the trigone necessarily carries with it the terminal portions of the ureters and this traction and torsion may lead to partial obstruction of the ureteral lumen, resulting in a hydroureter or hydronephrosis. It is a common experience to find catheterization of one or both ureters difficult or even impossible in cases of large cystocele and prolapsus uteri since the catheter tip meets an obstruction one or two centimeters from the meatus, but on reducing the cystocele by inserting a tampon in the vagina, the angulation of the ureter is removed and catheterization proceeds without difficulty. The demonstration of such an obstruction by no means indicates a functional impairment of the ureter, for in several of these cases we have shown by pyelograms the angulation of the ureter with a normal lumen above it. On the other hand, a potential obstruction is present, which, with increase in the mechanical factors or in the presence of infection, may be converted into an actual obstruction, giving rise to the typical symptoms of hydronephrosis or pyelitis as the case may be. Such symtoms often show a definite relationship to menstruation and are due to the increased edema of the trigone which serves as an additional factor in occluding the intravesical portion of the ureter.

In concluding this portion of our paper, we would refer to the association of vesical symptoms with an uncomplicated anteflexion or retroflexion of the uterus. Contrary to the generally accepted teaching, we are of the opinion that the malposition *per se* is rarely, if ever, productive of vesical irritability, for our experience has shown that the origin of such symptoms is to be explained by a lesion along the

urinary tract, and is in no way dependent upon pressure which the uterus may make upon the bladder base.

With this résumé of vesical lesions which may arise as the direct result of pelvie pathology, let us pass to another and by no means less important phase of the subject, viz., the recognition of complicating lesions within the urinary system, the pathology of which is entirely independent of that within the pelvis. The symptoms of both may be limited to those of deranged bladder function and here again their interpretation must depend entirely upon cystoscopic studies. In many instances incomplete and, indeed, often unnecessary, operations had been performed upon these patients due to hasty conclusions based merely upon the clinical history and pelvic examination.

In conclusion, we cannot emphasize too strongly the importance of making cystoscopic examinations in all gynecologic cases presenting vesical symptoms, even though the condition may seem quite sufficient to explain these symptoms. Not infrequently, by such a plan, lesions will be discovered which were unsuspected and which are of more vital import than those of the pelvic organs.

MEDICAL ARTS BUILDING.

(For discussion, see page 428.)

CERTAIN DIETARY FACTORS IN THE CAUSATION OF STERILITY IN RATS*

By Edward Reynolds, M.D., and Donald Macomber, M.D., Boston, Mass.

A NIMAL breeders have long held that glossy coat, clear eye, firm muscles, etc., were essential to successful reproduction and have attempted to obtain this state, which they call "good breeding condition," by grooming, graduated exercise, and special diets which, in all species, they habitually make high in proteins.

Our clinical experience with sterile human couples early led us to notice the frequency of poor general condition in one or the other of the partners and brought us to the question of how far many sterilities may be due to the evident absence of "good breeding condition" in these individuals. Reading along this line led us to the study of the dietary work of McCollum, Osborne and Mendel² and others. We noted their observations that diets deficient in certain important elements have resulted in lowered fertility. These elements are the mineral salts which are characteristic of the ash of all animals, of which

^{*}Read at the Forty-sixth Annual Meeting of the American Gynecological Society, Swamp-scott, Mass., June 2 to 4, 1921.

Acknowledgement is made of financial aid received through the kindness of the Trustees of the Elizabeth Thompson Science Fund.

calcium is the most important, a sufficient amount and variety of the proteins, and the substances which are generally known by the somewhat unfortunate name of vitamines and are classified as the fat soluble A vitamine, the water soluble B vitamine, and the antiscorbutic vitamine.

Personal interviews with McCollum and Mendel, their suggestions, and still kinder offers of assistance, then led us to the experimental work on which this paper is founded.

We were fortunate in obtaining an opportunity to work in the genetic laboratory of Castle at the Bussey Institution of Harvard University and gladly acknowledge our great indebtedness to his advice and instruction.

The deficiency diets were planned for us by McCollum to produce sterility without marked loss of health in adults or extreme underdevelopment in growing individuals and the general condition of the animals so dieted has been watched carefully by weight charts (Figs. 6 and 7).

The fertility or infertility of the animals so treated has been established by matings and rematings under varying conditions and from time to time the testicles and ovaries from individuals on each diet have been subjected to serial section and histologic study. Here we must express our gratitude and indebtedness to Bremer of the Harvard Medical School in whose laboratory and under whose direction the histologic work has been carried out. The work is still in progress, but has already yielded some results which will be touched on later (Figs. 8 to 14).

The composition of the diets used is shown in Table I and their analyses in Tables II and III, but lack of space forbids a full discussion of their details in the text. All the diets were fed in unlimited amount, an ample supply being continually before the animals. The stock diet of the laboratory on which Castle's rats have shown an exceptionally high fertility, and which we used as a check diet, differs from the others in that it offers a certain element of choice in accordance with individual appetites, being always present in two forms, a powdered dry mash and a bread, or cake, similar to dog bread. The rats consume on the average equal quantities of each and the analysis of the stock diet as a whole in Table II, is based on this fact. The deficiency diets were fed in powdered form only (dry mash) and were therefore invariable. In selecting a low percentage of the fat soluble A vitamine, of calcium and of proteins for the three diets to be experimented with we were guided by the fact that these are the three vital substances in which deficiencies are most frequently observed in human beings in this country. The deficiencies in the water soluble vitamine and the antiscorbutic vitamine are rare in western civilized life and are more-

TABLE I
COMPOSITION OF DEFICIENCY DIETS

DIET LOW IN FAT SOLUBLE VI	TAMINE	DIET LOW IN CALCIUM		
Rolled oats	40.0	Whole wheat	67.5	
Gelatine	10.0	Casein	15.0	
Casein	5.0	Whole milk powder	10.0	
Salt Mixture (McCollum)	3.7	Sodium chloride	1.0	
Dextrine	40.3	Inert substance	1.5	
Cod liver oil	1.0	Cod liver oil	5.0	
DIET LOW IN PROTEINS	-	DOUBLE DEFICIENCY (WAR)	DIET	
Wheat	70.0	Wheat	60.0	
Salt mixture (McCollum)	3.7	Sodium chloride	1.0	
Dextrine	21.3	Inert substance	1.5	
Cod liver oil	5.0	Dextrine	32.5	
		Cod liver oil	5.0	

It will be seen that each diet in this table contains an ample supply of all essential elements with the exception of the one for which it is designed to be deficient.

TABLE II
CHEMICAL ANALYSIS OF DIETS

8	STOCK DIET	LOW FAT SOL. VITAMINE	LOW CALCIUM	LOW PROTEIN	OR WAR DIET
Protein	18.0	19.6	23.4	8.4	7.1
Fat	10.1	3.8	6.7	6.5	6.3
Carbohydra	ate 55,3	63.1	53.6	69.5	72.3
Salts	4.7	4.5	2.6	5.0	2.1
Fiber	0.9	0.9	2.7	1.3	2.6
Moisture	11.0	8.1	11.0	9.3	9.6

By comparing the various diets as to chemical constituents it will be noted that they are all adequate except for the designated deficiencies.

TABLE III VITAL ANALYSIS OF DIETS

(AMP)	OCK DIET LE IN ALL MENTS)	LOW FAT SOL. VITAMINE	LOW CALCIUM	LOW PROTEIN	(LOW IN CALC. AND PROTEIN)
Protein	18.0	19.6	23.4	8.4	7.1
Fat. Sol. Vit.	5.0*	1.0*	5.0*	5.0*	5.0*
Calcium	1.8	1.88	0.03	1.88	0.03
The amou	nt of the	fat soluble vit	amine cannot b	e determined b	v chemical analysis.

The figures given represent the amount of the vitamine containing substance.

over disease-producing. They were therefore omitted. To the three diets selected we added a fourth, a diet deficient in both calcium and protein, on the ground that it is at least a partial representation of the war and postwar diets from which portions of Europe have been, and are even now, suffering. This double deficiency is therefore of human interest at this moment.

The rat was selected as the experimental animal mainly for convenience, as an omnivorous animal with a digestion closely comparable with that of our own species, and as being singularly free

^{*}Estimated.

from any of the diseases or abnormalities which produce sterility. This makes it especially useful for a study of the production of the functional decreases of fertility which was the object of this work. We used a strain of Albino rats which had been inbred by brother and sister matings with selection for health and strength for thirty-four generations by Helen D. King³ of the Wistar Institute. This strain is especially valuable for experimental work on account of its extreme genetic uniformity. King reports that 80 per cent of their matings have been fertile in her laboratory where the diet is very varied. On the stock diet of Castle's laboratory without selection it has been 65 per cent.*

These rats were subjected to the deficiency diets without any other change in their life conditions. Rats on each of the diets were then bred together and the results were checked by breeding the same individuals with rats of known previous fertility from Castle's stock. As more than 96 per cent of Castle's rats are fertile, individuals of previously proved fertility from this strain have been here considered as possessing 100 per cent fertility.

TABLE IV

RESULTS OF MATINGS ON VARIOUS DIETS (KING X KING RATS)

DIETS		NO. OF MATINGS	NO. OF POSI- TIVE MATINGS	PERCENTAGE OF MATING FERTILITY	PERCENTAGE OF INDIV. FERTILITY
Stock diet		23	15	.65	.81
Low Fat Sol.	Proved	S	4	.50	.70
Vitamine diet	Unproved	l 13	4	.31	.55
Low Calcium diet	Proved	0	0		
Low Protein	Unproved	1 7	1	.14	.37
diet	Proved	0	0		
Double Defi-	Unprove	1 5	0		
ciency diet		3	0		
(Low in calcium and protein)	Unproved	d 5	0		

This table shows the result of matings in which both partners were King rats and both were on the indicated diet.

We think that our experiments have already shown that dietary deficiencies which are too slight to produce ill health or even marked loss of condition nevertheless result in a high percentage of infertile matings; they also show that many of the individuals which compose these infertile matings are capable of reproduction when remated to highly fertile individuals; they suggest that the degrees of infertility

^{*}It should be noted that King in maintaining her strain has bred from her best rats, while we, necessarily, bred all our rats without distinction.

so produced can be measured with fair accuracy; and that a threshold of fertility below which reproduction does not occur can be approximated. Complete proof on these two latter points will, however, require larger numbers than we have so far employed.

The effect of these diets on the fertility of the individuals concerned is given in percentages in the column headed Mating Fertilities in Table IV. We intended to test all the deficiency diets both on rats which had been previously fertile when on stock diet, and on rats reared to maturity on the experimental diets, but accidental deaths and other laboratory accidents in connection with the small supply of stock available at the start made the number of previously fertile rats

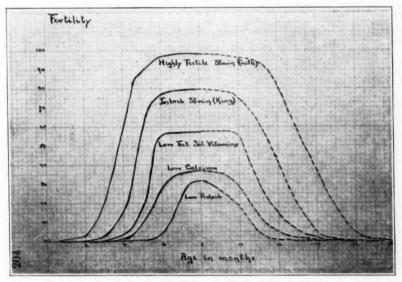


Fig. 1.—This chart combines percentage of fertility with the age at which reproduction first occurred for Castle rats; for King rats; and for rats on the three single deficiency diets. The dotted half of each curve does not represent actual observations, but conforms roughly with the sexual life of these animals. The sharp rise corresponds to puberty and the sharp fall to the cessation of sexual activity. Note that the effect of deficient diet is to shorten and depress each curve as a whole.

which were mated on the two diets which were deficient, respectively, in calcium and in protein, too small to warrant a report. These results are also shown graphically in Fig. 1 in which the curves show both the age at which fertility occurred and the degree of fertility which was finally attained on each diet.

When the diet deficient in the fat soluble vitamine was given to rats which had been fertile while on the stock diet, it produced a decreased fertility which is probably too slight to be used statistically from so small a number of matings. It is included in the table for what it is worth, but will not be discussed further. When, however, young animals were reared to maturity on this diet and then mated, their fer-

TABLE V
RESULTS OF RE-MATINGS ON VARIOUS DIETS (KING X CASTLE RATS)

DIETS		NO. OF MATINGS	NO. OF POSI- TIVE MATINGS	PERCENTAGE OF MATING FERTILITY	PERCENTAGE OF INDIV. FERTILITY
Stock diet		10	7	.70	.70
Low Fat Sol.	Proved	0	0		
Vitamine diet	Unproved	1 20	9	.45	.45
Low Calcium diet	Proved	0	0		
Low Protein	Unprove	d 19	6	.30	.30
diet	Proved	0	0		
Double Defi-	Unprove	d 12	6	.50	.50
ciency diet		0	0		
(Low in ealcium and protein)	Unprove	d 0	0	0	0

This table gives the results of remating rats from the negative matings of Table IV with Castle rats of known fertility. Since such Castle rats are of practically 100 per cent fertility the index of average individual fertility of their King rat partners is the same as the index of mating fertility. It will be seen that the individual fertilities of rats on the deficiency diets are so low as to have insured sterile matings with partners of the same grade (see Table VI).

tility was decreased by more than one half (0.31 as compared with 0.65). (See Table IV.)

The diet which was deficient in calcium and the diet deficient in the proteins both decreased fertility to an amount which is noteworthy even from a limited number of experiments, as is shown in Table IV.

In the rats subjected to the double deficiency diet (decreased calcium and protein) there were no pregnancies at all either in previously fertile adults or in rats reared to maturity on the diets.

The results were then checked by remating these individuals with rats of proved fertility from Castle's highly fertile stock as shown in the column of mating fertility in Table V. The results obtained were at first sight altered, but this is due to the high fertility of the partners. They were in fact confirmatory of the percentages given in Table IV, as will be apparent on discussion of the percentages of "average individual fertility" to be taken up now.

Study of these results made it at once evident that in any discussion of the sterile matings produced we must make a distinction between the "mating fertility" of a class and the "individual fertility" of the several animals concerned.

The percentages of individual fertility given in Tables IV and V of course represent the average of individual fertility throughout each class, and this must again be distinguished from the individual fertility of given animals which can only be arrived at by individual testing.

Let us consider the results in detail. On the calcium deficiency diet

6 out of 7 matings were sterile, i.e., the class showed 14 per cent of fertility as compared to 65 per cent on the stock diet. This diet was deficient only in its percentage of calcium and was otherwise an abundant and nourishing diet. It did not produce visible ill health.

On the protein deficiency diet there were only 5 matings and no pregnancy occurred. It is probable that on a larger number of matings there would have been an occasional positive mating and a possibility of calculating their rating. This is supported by the fact that some of these rats were subsequently fertile to rats of higher fertility (Table V).

On the double deficiency no pregnancy was ever obtained in these matings.

The production of so much infertility by such slight change in conditions is at first surprising, but it is rendered much more comprehensible by the theorem which we now wish to bring forward, that a slight decrease of fertility in each of the individuals concerned produces a much greater decrease of fertility in the matings.

If in this connection we make certain assumptions, calculate from them the fertilities which should theoretically be produced, and on turning to the actual results of the breeding experiments, find that the same, or closely similar, percentages were in fact obtained, we may fairly conclude that the assumptions are sustained, in so far at least as the small numbers used warrant any conclusion.

We will assume then that the fertility of a mating may be fairly expressed as the product of the individual fertilities of the individuals concerned, and in written form by the formula

The fertility of the male x the fertility of the female—the fertility of the mating or, for brevity — m x f — M

TABLE VI SCHEMATIC CHART OF FERTILITY

INDIVIDUAL	DIVIDUAL FERTILITY		MATING FERTILITY		
1.0 x	1.0		1.00	or	100%
.9 x	.9		.81	or	81%
.8 x	.8		.64	or	64%
.7 x	.7	Threshold for	.49	or	49%
					45%
.6 x	.6	Reproduction	.36	or	36%
.5 x	.5	*	.25	or	25%
.4 x	.4		.16	or	16%
.3 x	.3		.09	or	9%
.2 x	.2		.04	or	
.1 x	.1		.01	or	1%

The figures in this table vary from those actually obtained from the matings only in being restricted to even tenths for the sake of clearness. The establishment of .45 as the threshold for reproduction must of course be regarded merely as an approximation until larger numbers have been employed.

We will assume that when individuals of the same parentage have been reared under the same life conditions and subjected to the same change in those conditions, their fertilities will be affected in substantially similar degree whether the individual is male or female. Then if in a given class we make 10 matings, of which 5 are fertile, the aver-



Fig. 2.-Stock diet.



Fig. 3.-Low fat solution. Vitamine diet.



Fig. 4.-Low calcium diet.



Fig. 5.-Double deficiency on war diet

age mating fertility of this class would be 5 and the formula would be $m \times f = .50$, but if in this formula both individuals are of equal fertility it follows that the fertility of either individual is equal to the square root of .50. The square root of .50 is approximately .7

 $(.7 \times .7 = .49)$; hence if our assumptions are correct the formula for a class in which 10 matings give 5 fertilities would be $.7 \times .7 = .5$.

Study of a number of such formulæ leads, however, to a further theoretical conclusion,—that somewhere in such a series there must be a point above which reproduction may be expected, but below which it will not occur, a "threshold for reproduction" (Table VI).

If we now apply these formulæ to the 23 matings of Dr. King's rats on our stock diet we obtain the following results. There were 31 individuals involved in these 23 matings. In the case of 22 individuals the first mating resulted in prompt reproduction. We may safely conclude that these individuals were of the average mating fertility of

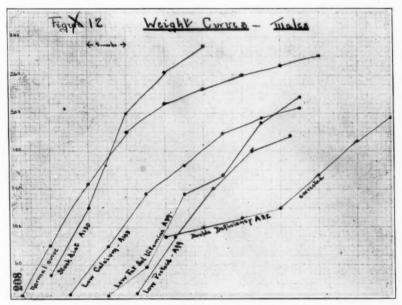


Fig. 6.-Individual weight curves for males.

King's strain which she gives as .8 (the square root of .8 is .9, $.9 \times .9 = .81$) which may then be taken as the individual fertility of these 22 rats. The other 9 individuals all eventually reproduced, but in their case the number of matings required before a result was obtained varied from 2 to 7. Estimating their individual fertilities by the same method, i.e., by the percentage of mating fertility in the whole group of matings in which the individual was concerned, and taking the square root of the mating fertility as the fertility of the individual, adding the individual fertilities of the whole class and dividing by 31 we get an average individual fertility of .82 for the whole group of 31 rats. If these assumptions are correct, or nearly so, this figure (.82) should approximate that obtained by taking the square root of .65

which was our observed index of mating fertility in this class (Table IV). This square root is .81, and the comparison is close enough to warrant the assumption.

The same method was pursued in the establishment of a threshold for reproduction. Taking the total number of matings in which any of these rats figured, which is 33, the appropriate individual index was introduced into the equation for each mating. This gives us a table of mating fertilities ranging from .28 to .90. In some of these matings young were produced; in others there were no young, or, as we have phrased it, some matings were positive and others negative. The column of mating fertilities in Table VI gives in schematic form the results obtained. In practically all the matings which fall above the dotted line young were produced; in practically all those which were below the dotted line the matings were infertile.

In fact, if we so place this "threshold for reproduction" at a mating fertility of .45 there were but two exceptions to the rule. In one case young were produced in a mating with an index of .40, but in this case it was the seventh mating to which that individual rat had been subjected, the six others being infertile. In one case a mating with an index of .49 did not produce young, but this mating was a brief one. It was made and terminated before this point had come into our minds and might have proved fertile if it had been longer continued.

We fully realize that this theory must be marked unproved until it has been tested by a much larger number of matings. We are already engaged, in conjunction with Castle, in repeating these experiments on a scale large enough to be evidential, and shall report the results with him in a later paper. We would only say here that if it is reliable it explains very many otherwise inexplicable facts; for instance, the often repeated observation that two rats mated with each other failed to produce young, but that when each was remated with a partner of higher individual fertility (e.g., a Castle rat) both matings proved positive.* On this theory the explanation is evident. Suppose the rats concerned in a class each had an index of individual fertility of .6, the mating fertility in that case would be .36, which is below the threshold for reproduction and no young would result; but in their rematings with rats whose individual fertility was 1.0, the mating fertility in each case would be .6 and young would result. Again it makes it easier to understand the otherwise extraordinary fact that a merely decreased percentage (not an absence) of calcium without any other change in the conditions of life can (at least in rats) make 6 out of 7 matings sterile, (or the similar results in the other classes). It has long been known in the laboratories that an-

^{*}Thirty-seven individuals which had had negative matings reproduced with new partners,

imals subjected to highly deficient diets become sterile before they develop ill health. These experiments show that even moderately deficient diets decrease fertility and if this theory is true it explains how a moderate decrease of fertility in each of two individuals must make their mating sterile, as, for instance, $.6 \times .6 = .36$ or sterility.

It is, of course, to be remembered that if a class shows a mating fertility of about .36 this means that 4 matings out of 10 have been fertile. The explanation of the occurrence of these 4 fertile matings with an average individual fertility of only .6, of course, means that with an average fertility of .6 some individuals will be at .7 or higher and that whenever two such individuals meet their mating will be

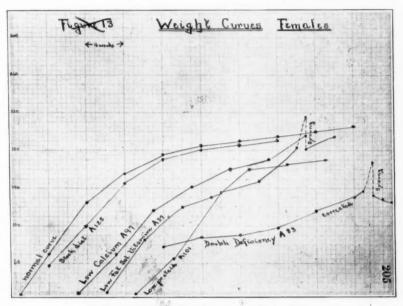


Fig. 7.-Individual weight curves for females.

fertile. Other individuals will be as low as .5 and would probably be sterile when mated with even the best of the class, e.g., $.8 \times .5 = .4 =$ sterility.

One other observation of some interest may be mentioned here. We have been unable to find any mention of the birth of macerated fetuses in the literature and Castle is sure that he has seen no such instance in the many years during which his laboratory has contained large numbers of rats on stock diet. In 32 matings on our single deficiency diets 4 such deliveries occurred and there were 2 more in 8 deliveries in which young rats which had been reared on the war diet were subsequently placed on ample diet and bred to Castle's rats. The possibility that habitual abortion in the human race may be the result of one of the deficiencies in these vital ingredients of diet which are

not infrequently present as a result of faddism or lack of appetite, even in the well fed classes, should be given consideration. We have observed one instance in which five successive abortions occurred in a

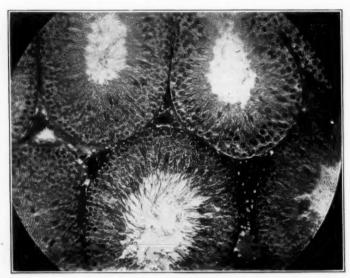


Fig. 8.—Section of testis (x500) from rat made sterile by low fat solution. Vitamine diet.



Fig. 9.—Section of ovary (x300) from rat made sterile by low calcium diet.

woman whose diet when analyzed showed such a deficiency, but in whom a sixth pregnancy on corrected diet resulted in a normal child at term. The work of Steenbock⁴ of the Wisconsin Experiment Station shows that calcium deficiency tends to produce abortion and stillbirth in swine and cattle also.

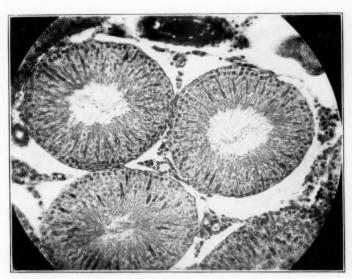


Fig. 10.—Section of normal testis (x500).

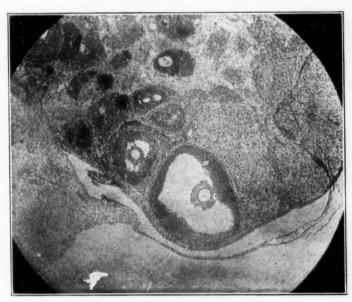


Fig. 11.—Section of normal ovary (x300).

We have stated that the deficiency diets produced no disease or evident ill health (with the exception of the war diet which did produce wasting and in several instances death). Figs. 2 to 4 are photographs

of representative rats from each class and are the best evidence of this fact which we can produce on paper. The condition of representative individual rats in each class is further shown by Figs. 6 and 7 which give their individual weight curves. It will be seen that on the single



Fig. 12.—Section of ovary (x300) from rat made sterile by war diet.

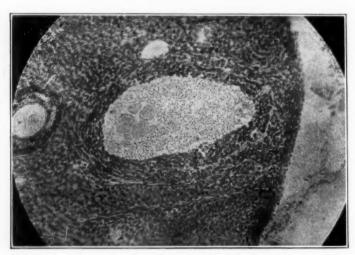


Fig. 13.—Section of ovary (x500) from rat made sterile by war diet. Note the four cells in the follicle.

deficiency diets these vary extremely little from the average normal curve of rats and from the slightly better curve obtained from our stock diet.

The photographs (Fig. 5) and the individual weight curves of rats on the double deficiency diet (low in calcium and protein) show these rats to be in less good health, and this marked change of condition was attended by great loss of fertility, all their matings with each other proving sterile. We have obtained European statistics which show a corresponding decrease in human fertility in the countries of central Europe, notably in Austria, but lack of space forbids the reproduction of these statistics in this paper.

It seemed necessary for completeness that we should study the ovaries and testicles of rats which had been decreased in fertility by these diets. Since we were able to find no extended study of the physiologic condition of the ovaries and testicles of rats in a state of fertility our study of these organs in rats on deficiency diets was necessarily pre-

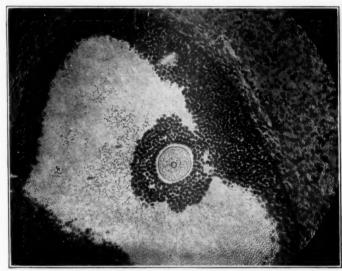


Fig. 14.—Section of normal ovary (x500).

ceded by the preparation of serial sections of the entire ovary and testicle from stock diet rats in every month from birth to full maturity. With these as standards we compared serial sections of the organs from representative rats in each of the classes which were on deficiency diets. The work of Robinson⁵ on prenatal death makes it probable that certain ova, and probably certain spermatozoa, contain within themselves conditions which make complete development impossible; but these conditions may, or may not be, morphologically visible.

Figs. 8 and 9 are sections from the organs of rats on the single deficiency diets. We exhibit them with the statement that when compared with sections from normal animals (Figs. 10 and 11) they show no anatomical changes which have been demonstrable under the methods so far employed.

Figs. 12 and 13 are sections of ovaries of rats on the double deficiency diet with their more extreme infertility. These ovaries do show several peculiar conditions which must be studied further, notably ova contained in unruptured graafian follicles which are composed of four cells. This section is to be compared with a normal ovum under high power (Fig. 14). These may be ova which have undergone premature segmentation, they may represent the development of four ova in a single follicle, we are uncertain what they represent and can at present make no statement about them except that they are plainly abnormal.

CONCLUSIONS

We conclude provisionally: 1. That a moderate decrease in the percentage of the fat soluble vitamine, of the protein, or of the calcium contained in an otherwise excellent diet produces a definite decrease in the fertility of individual rats.

2. That a slight decrease in the fertility of both partners will produce a sterile mating.

3. That the fertility of the mating may be stated as the product of the fertility of the individuals concerned.

4. That if the index so obtained falls below a given point the mating will be sterile, and that this result holds true whether the partners are of equal or of widely different fertility.

5. That these principles explain the fact that two individuals which are sterile when mated together may nevertheless reproduce freely when mated to new partners (of higher fertility).

6. That dietary deficiencies produce a lowered fertility which varies in degree with different individuals though of the same parentage and in the same cage.

7. That diminished fertility sometimes results in the appearance of abortion.

8. That mere percentage deficiency in both proteins and calcium produces visible ill health and great infertility.

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321 DARMOUTH STREET.

(For discussion, see page 431.)

A TECHNIC FOR THE MANAGEMENT OF THE LARGE CYSTOCELE WHEN ASSOCIATED WITH NON-MALIGNANT DISEASE OF THE CERVIX AND MYOMATA UTERI

By LILIAN K. P. FARRAR, A.B., M.D., F.A.C.S., NEW YORK CITY (From the Clinic of the Woman's Hospital.)

THE recognition that cystocele is a true hernia of the bladder and that its repair rests upon the same principles governing surgical procedures for the cure of hernia elsewhere in the body, is a development in gynecologic surgery of the past thirty years. Sims (1871) was the first to completely expose the base of the bladder as it is done today, but he did it quite by accident, and did not employ this method again.

The etiology of the occurrence of cystocele and the principles of fascial support of the bladder we owe to Hadra (1888) and to him belongs the credit for first suggesting in 1889 the freeing of the bladder from the cervix and anterior vaginal wall and "resuturing the bladder to the anterior cervical lip as high up as possible." Hadra considered the point on the cervix where the bladder and vagina are firmly attached to one another to be of especial importance—"as a centre upon which the connection and the mutual support of the three organs depend—and the reattachment and restoration of normal relations between the bladder, vagina and cervix is what we ought to look for." Hadra apparently did not perform the operation, for, after giving this technic, he says "I have had no experience in it but I can promise that the following little operation answers our purpose very well," and describes passing a suture through the vaginal tissue on either side of the cystocele and fastening to the cervix, a procedure similar to that employed by Emmet.

The first, however, to appreciate that a prolapse of the bladder is a true hernia seems to have been Munde, who, in 1890, in an article in the American Journal of Obstetrics, of which he was then editor, entitled "True Cystocele or Vesicovaginal Hernia," says that "stitches should include separated muscular fibers of the anterior vaginal wall instead of merely drawing together the vaginal mucous membrane." Two years later Skene in "Hernia of the Bladder" gives credit to Munde for this conception of cystocele as a bladder hernia and describes an operation which he had performed on the principle for

repair of hernia. The freeing of the bladder was, however, limited "to the extent of the hernial opening in the muscular layer of the vagina," but the lateral edges of the muscular layer of vagina he brought together with sutures. This, so far as I have been able to find in the literature, is the first operation undertaken for cystocele with the knowledge on the part of the operator that he was dealing with a hernia and the intent to effect a cure by freeing the bladder from the vaginal wall and then bringing together the separated muscle in the wall of the hernial sac.

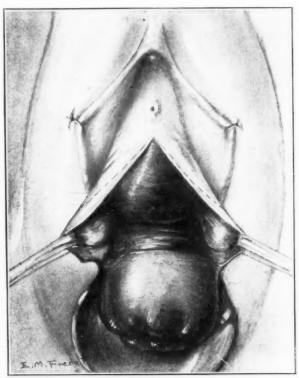


Fig. 1.—Cystocele associated with diseased cervix and myoma of the fundus (with or without procidentia).*

The operation of "vaginofixation" associated with the names of Mackenrodt, Sänger, Dührssen, Schücking and others was the fore-runner of the interposition operation and the improved operation of cystopexy. Mackenrodt did the operation first in 1888 by passing a suture through the cervical canal and fundus of the uterus to the vesicouterine attachment and vagina without freeing the bladder. As the bladder was in danger of being injured by this stitch, Mackenrodt later made the opening of the vesicouterine peritoneum a preliminary step to the operation, but never freed the bladder extensively.

^{*}Figs. 1, 2, and 3 are after Ward's technic for cystopexy and amputation of the cervix. Fig. 4 from Martin's Heftapparat.

The next in order chronologically seems to have been the "flap" splitting method of Sänger." The technic of flap splitting was first applied to the repair of the pelvic floor by Lawson Tait, and by Sänger somewhat later. Sänger applied this technic to the anterior vaginal wall and the operation generally bears his name, but he himself in 1892 gives credit to Gersuny and Arx for being the pioneers in this work. While Sänger approved of bladder separation from the anterior wall of the vagina far out laterally on either side, he disapproved of its separation from the uterus and in this point differed from both Arx

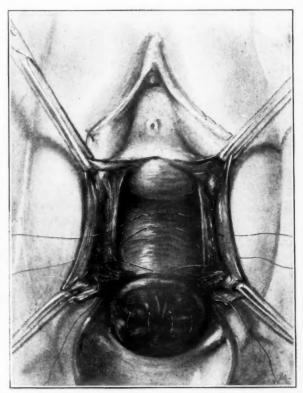


Fig. 2.—Freeing of the bladder and cutting off the thinned-out sac. Amputation of the cervix. Alexandroff's stitch and angulation of the vagina. (Note that the bladder is not attached to the anterior surface of the uterus, but left free.)

and Gersuny who separated the bladder from the uterus but not so high as the peritoneal attachment which they left intact. Arx is the first to mention the elevation (Einstülpung) of the bladder as an important step in the operation but he accomplishes this by turning the bladder mucosa upward into a ridge or cone within the bladder after the method of Emmet. Gersuny and Martin both employed this infolding of the bladder wall which they maintained by interrupted sutures.

The first description of an operation performed after the technic

suggested by Hadra was that given by Stone, of Washington, (1889) who separated the bladder from the uterus and after excising the thinned-out portion of the vaginal wall sutured it to a point on the anterior surface of the uterus on a level with the origin of the round ligament and then opened the abdomen and made "a further separation of the bladder from the uterus and sutured the reflexure to the scarified surface of the uterus near the fundus." If he considered it advisable, he also did a ventral fixation.

It has been of no little interest to me to trace the origin of the transposition operation or as it is known in German literature, the interposition operation. I was a student in Vienna when the discussion was

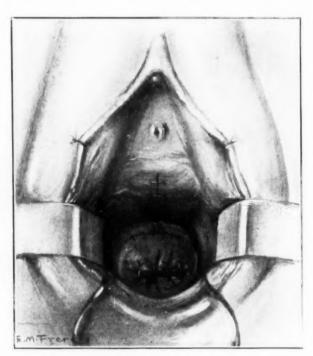


Fig. 3.-Vaginal part of the operation completed.

keenest as to whether the credit for priority belonged to Schauta or Wertheim, and as I was for several months in the clinics of Schauta at the Krankenhaus and also in the clinic of Wertheim at the Kaiserin Elizabeth Bettina Pavilion, I had ample opportunity to hear this discussion from both sides,—but I never heard any credit given to an American surgeon. As the operation is known today in German text books as the Schauta-Wertheim operation, I think it may not be amiss to present the facts in detail.

In 1894 Dührssen reported a series of 250 cases upon whom he had done a "vaginofixation" operation. I mention this because Dührssen

has been credited sometimes as being the first to do the transposition operation, but there is nothing in his report to warrant this assertion. Dührssen did incise the vesical peritoneum frequently to avoid injuring the bladder, but his technic is that of Mackenrodt in vaginal fixation of the fundus of the uterus in cases of retroversion.

In 1896 Freund, when operating for prolapse, made a posterior colpotomy incision and brought the fundus through the opening into the vagina and fastened it to the walls of the vagina. The uterus thus



Fig. 4.—The bladder has been completely freed from its vaginal attachments. (Note stitch plicating the tissue and attaching the bladder to the cervix.)

lay in the vagina without any vaginal covering. Freund then made an incision in the fundus for the purpose of drainage.

On January 28, 1898, Thomas J. Watkins, of Chicago, performed a transposition operation precisely as it is done today, bringing the fundus of the uterus below the bladder, fastening it there and suturing the vaginal flaps anterior to the fundus. This operation was reported in the American Gynecological and Obstetric Journal for 1899 (xv, 420).

On January 10, 1899 (this is one year later than Dr. Watkin's operation) Wertheim, who said he had in mind Freund's technic, when

operating to cure a large vesicovaginal fistula, brought the uterus into the vagina by an anterior incision and fastened it under the bladder and anterior to the vaginal flaps, so that it lay uncovered in the vaginal canal. On January 18, 1899, Wertheim employed the same technic for a large cystocele associated with prolapse. This operation Wertheim reported in the Zentralblatt für Gynäkologie in April, 1899.

Some time after this Dr. Josef Halban (who was then assistant to Schauta, at that time chief of the Gynecologic and Obstetric clinic) when operating upon a patient according to Wertheim's technic, and at the suggestion of Schauta who was present in the operating room, fastened the vaginal flaps anterior to the fundus of the uterus, as Watkins had done in his first operation. (This was a personal communication from Dr. Halban to me while I was working in the clinic.) The credit therefore for this operation belongs neither to Schauta nor to Wertheim, but belongs rightfully to Watkins who antedates them both in the transposition of the bladder and the attachment of the vaginal flaps; and the operation should be known as the Watkins' transposition operation.

Löwit in 1909 reported a transperitoneal vaginal supravaginal resection or amputation of the body of the uterus and utilized the cervix, (by sewing it into the vesicovaginal septum), to support the cystocele after the manner of a Watkins operation.

In 1915 Vineberg amputated high up the body of a chronically fibrosed uterus, and, after amputating the lower portion of the cervix, utilized the midcervical portion or stump by suturing it to the subpubic fascia and to the anterior vaginal wall, thus forcing the bladder "to take a position within the abdomen."

In 1902 Alexandroff described the technic, now known by his name, of placing a stitch through the base of the broad ligaments on either side of the cervix drawing them forward and fastening low down on the anterior wall of the cervix, thus making a buttress of them and elevating and forcing the cervix back in the pelvis, a method described later by Tweedy.

In 1901 Reynolds laid down the "principles underlying the repair of cystocele or governing repair of hernia elsewhere," viz.:

1. To ascertain and utilize the natural supports.

2. To avoid using any part of the overstretched wall. Excision of the thinned-out portion of the anterior wall was done by Noble in this same year, 1901, and Dudley, 1903.

In 1904 Goffe more thoroughly applied the principles employed in repair of hernia and freed the bladder completely from its cervical attachment, overcorrecting the prolapse of this organ by elevating the bladder base and reattached it high up on the anterior wall of the uterus, and broad ligaments, excising the thinned-out portion of the vaginal sac with the intent to

- 1. Provide good support for the bladder below.
- 2. Restore suspensory supports above.
- 3. Do away with redundant folds of bladder wall at its base.

This spreading out of the trigone of the bladder and its attachment to the face of the uterus and broad ligaments insures, not only a firm supporting surface, but prevents infolding of the bladder wall, as kinking of the ureters with stagnation of urine and consequent irritation of bladder mucosa or cystitis.

In 1907 Noble advocated this same technic and by sutures placed in

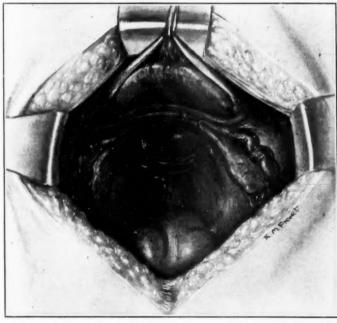


Fig. 5.—Supravaginal hysterectomy has been done and the round ligaments (and the tubes and ovaries if not removed) fastened anteriorly to the stump of the cervix at the same level and to the uterosacral ligaments posteriorly to form a platform upon which to spread out the bladder.

the cervix directly below the bladder gave more firm support the base of the bladder wall, a method reported in 1913 by Lockyer.

Edouard Martin in 1912 in an anatomic study of the genitalia demonstrated the pubocervical ligament (bladder pillars), the importance of which have been confirmed by Frank in 1917 and Rawls in 1918 in their work on cystocele. The reattachment of the vaginal wall together with this ligament restores the invagination of the cervix to normal or to an overcorrection, as in hernial repair elsewhere.

The overlapping of the fascia below the bladder was first reported

by Rawls and Bissell and to the latter and to Mayo should be accredited the operation of uniting the broad ligaments and placing the bladder above after the complete removal of the prolapsed uterus.

The recognition of the supporting and elevating power of the uterosacral ligaments somewhat antedates the above as attention was called to this factor in restoring prolapse or displacement of the uterus by Wertheim and Mandl, Boyée and Noble.

So far mention has only been made of the repair of the cystocele from the vagina, but as early as 1890 the cure of the cystocele was sought by the abdominal method. Byford at this time operated

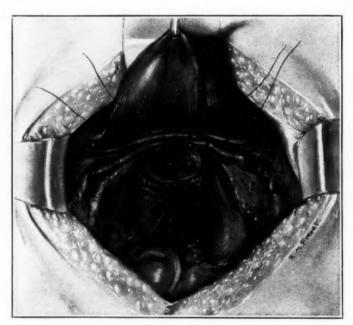


Fig. 6.-Two stitches tack the bladder to the anterior part of the platform.

through the inguinal canals, fastening the tissue on either side of the bladder to the incision.

Lawson in 1898 by suprapuble incision freed the bladder and sutured it to the sheath of the rectus.

In 1903 Dickinson suspended both bladder and uterus to the parietal peritoneum or by actual "fixation of bladder and uterus directly to naked muscle and fascia."

Polk in 1909 presented a technic for restoration from within the abdominal eavity of the prolapse of the uterus when associated with cystocele. The anterior wall of the vagina was plicated from above, and the uterus, if retained in situ, fastened well forward. If supravaginal hysterectomy was done the round and broad ligaments on either side were sutured to the stump of the cervix and the uterosacral

ligaments cut and after crossing them over the stump were sutured to its anterior surface.

In 1919 Ward in the "Problem of the Cystocele" gave a complete résumé of the salient points in the etiology of the occurrence of hernia. of the bladder and the technic for its repair, emphasizing the composite nature of the injury and the necessity to correct all the lesions in order that there "should be no weak link."

The technic of the operation I am about to present, so far as it pertains to the vaginal work, follows closely the method laid down by Ward and in the freeing and elevating of the bladder from within the abdominal cavity and the suturing of round and uterosacral ligaments to the stump of the cervix, is very similar to the technic of Polk, although I was not acquainted with the latter's work until I had done four cases.

Case 1.—Mrs. Q., W. H. 22949. Op. June 27, 1919; one hour and forty-five minutes. Alexandroff; combined vaginal and abdominal cystopexy; rectopexy; hysterectomy, supravaginal (platform); salpingo-ooprorectomy; shortening of uterosacral ligaments; appendectomy.

Case 2.—Mrs. I., W. H. 23676. Op. October 4, 1919; Alexandroff; combined vaginal and abdominal cystopexy; hysterectomy, supravaginal (platform); shortening of uterosacral ligaments; appendectomy.

Case 3.—Mrs. C., W. H. 25231; Op. February 20, 1920; 1 hr. 15 min.; Alexandroff; combined vaginal and abdominal cystopexy; op. for incontinence of urine (Kelly); rectopexy; hysterectomy, supravaginal, (platform); salpingo-oophorectomy, (bilateral); shortening of uterosacral ligaments.

Case 4.—Mrs. R., W. H. 27274; Op. Feb. 11, 1921; 2 hrs. 9 min., amputated cervix; Alexandroff; combined vaginal and abdominal cystopexy; rectopexy; hysterectomy, supravaginal, (platform); shortening of uterosacral; appendix previously removed.

INDICATIONS FOR OPERATION

Occasionally one meets with a case of large cystocele associated with a very much hypertrophied, badly torn cervix necessitating removal, and in the fundus large myomata, the removal of which by the vaginal route would be an impossible or difficult operation attended with great loss of blood and danger of infection. If in addition to this one finds on examination that there is no prolapse associated with the condition, it becomes impossible to reef the broad ligaments below and place the bladder above as in vaginal hysterectomy, or after complete hysterectomy from above to leave the bladder so supported that recurrence of the cystocele is not probable.

The following technic is therefore presented as having been satisfactory in four cases where an enlarged fundus and diseased cervix necessitated their removal, but the large cystocele associated required

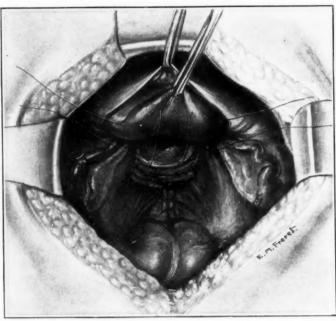


Fig. 7.-Two other stitches complete the attachment of the bladder to the platform posteriorly.

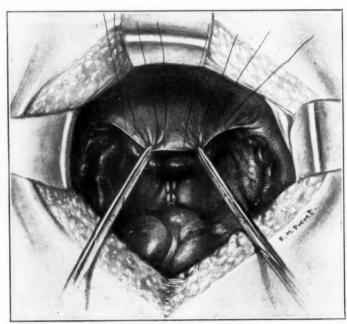


Fig. 8.--The free edge of bladder peritoneum (vesico-uterine fold) is sutured to the peritoneum to more firmly anchor the bladder in position.

a good support for its cure. A repair of the pelvic floor is an essential part of the technic although not shown in the accompanying illustrations.

CONCLUSIONS

- 1. This technic provides a good support for a cystocele when both fundus and cervix of the uterus must be removed.
- 2. The firm cervical attachments of the broad ligaments are maintained and the base of the broad ligaments, uterosacral and round ligaments are all utilized to secure pelvic support for the bladder.
- 3. Bladder is spread out over a platform which prevents any considerable degree of infolding of the bladder wall or kinking of the uterus.

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STERILITY STUDIES. SIMPLIFIED METHODS IN DIAGNOSIS*

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S IMPLICITY is art. If, therefore, by reviewing the facts adduced from several years of constant research in the subject of sterility we can finally arrive at a simplified method of studying the individual case which reduces the distasteful elements of such study to a minimum, both for the physician and for the patient, and also outline a short-cut procedure for determining the lesion interfering with conception, we shall feel that we have attained one of the essentials of a perfected technic.

The study of sterility has many discouraging features. Much clinical experience has been accumulated and often it is contradictory. Exceptions are hard to account for; many preconceived impressions are reluctantly abandoned, and, finally, the percentage of cures which we can honestly claim, is small. Two years ago I was visited by a gynecologist of another city who asked me why I had published so little lately on the subject of sterility. I replied, with some emphasis, that I was discouraged with the work, that I knew little about it anyway, and did not propose to write until I had something definite to say.

While the general results of my work still seem meager, spectacular success occurs from time to time, and a review of the individual cases during recent years discloses facts about results which are interesting and promising.

My study showed that by far the larger proportion of patients were hopeless from the start. But hope springs eternal regarding fertility. Many patients were older than forty. Once a physician of sixty-five from a neighboring state, came with his wife of fifty-two. Another large group is made up of cases of male deficiency, and this finding frequently terminates the case. Not a small proportion of cases were those presenting frank histories of gonorrheal pathology. Of the latter group I am at present taking a more hopeful view, for reasons which I expect to outline in a later paper. Still a smaller group, but one I approach with little enthusiasm, is that of patients who give a suspicious history of morbidity following a useless curettage. Curetting has no place in the treatment of sterility except as a means of examination when symptoms suggest a diseased endometrium. Constantly I hear in my office, "The doctor found nothing wrong with me so he said he would operate and scrape the womb." The members of this society do not need to be instructed, but as teachers and consultants

^{*}Read at a meeting of the New York Obstetrical Society, April 12, 1921.

there is a moral responsibility upon us to warn the profession and the public of the dangers of the curet. My effort tonight will not be in vain if I have contributed something toward spreading this important precaution. The analysis of these groups shows definitely that a better prognosis will be possible in sterility when the public and the profession are acutely aware of the need of early, competent study.

Of necessity one's work in sterility will not, therefore, be brilliant, and I advise and practice modesty in my prognosis to these anxious patients. Also I never tell a patient unreservedly that she cannot become pregnant (except when hysterectomy has been personally performed) for there is no mistake which may be so humiliating and disastrous.

The problem of sterility is usually first presented to the gynecologist and I think it entirely proper and practicable that the initial study should begin with the gynecologic history and examination. This is not the place to discuss such elemental matters as history taking, but I do want to emphasize that I see no reasonable excuse for including in such a history any unusual questions of an intimate or embarrassing nature. Long ago I concluded that the knowledge gained by such inquiry was not pertinent to the subject and there is no surer way to discourage the earnest cooperation of the patient, which is such a valuable asset in the work. No other comment will be made upon the history. In the physical examination more than usual stress should be given to evidence of underdevelopment and endocrine disorders.

THE EXAMINATION

Absolute classification of patients is impossible, but the gynecologic examination relative to sterility will disclose three general groups:

- 1. Cases with a gross pathology which entirely precludes pregnancy; or which renders pregnancy a dangerous complication. Also conditions which, while likely to cause sterility, have themselves an importance superseding sterility. This group of cases may be ruled out at once as not belonging to sterility problems.
- 2. The second group includes those patients with minor pelvic lesions, discovered incidentally, which have an importance due chiefly to the possible relation they bear to the interference of pregnancy. I refer to the flexions and uncomplicated versions of the uterus; so-called conical cervix with pinhole os; mucus plug obstructing the cervix; tilting of the cervix out of the seminal pool, et cetera. Much has been written as to their causative relation, but we are concerned with questions which actually determine their rôle in the individual patient. We may specifically state that the responsibility of such a lesion may be inferred only when the favorable character of all other essentials to fertility is demonstrated. When cases are thus studied, the contention of those who claim that these lesser pathologic condi-

tions are very rarely responsible for sterility will be confirmed. They have been considered responsible, in my opinion, largely, because they are so often associated with sterility as the symptom-complex of underdevelopment.

3. In the third group may be placed those patients in whom the examiner fails to find any abnormality.

THE INSEMINATION TEST

The next stage of the study, applicable to the last two groups, is conducted on the second visit. I term it the "insemination test," and it consists simply of studying the condition and motility of the

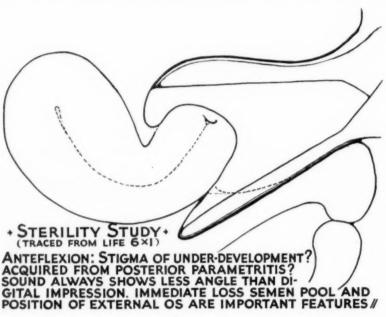


Fig. 1.

*Figs. 1, 2 and 3 are after Ward's technic for cystopexy and amputation of the cervix. Fig. 4 from Martin's Heftapparat.

spermatozoa in the reproductive tract of the recipient approximately forty minutes after coitus. Comparative findings are impossible without systematic study. This interval was originally specified as about the time necessary to permit the patient to reach the office. By test it was found also to present the most definitely favorable condition for study. I have found it practicable to set aside Sunday and one other morning a week for this work. The husband should be informed as to the nature of the investigation and a definite appointment for the wife's visit made a few days in advance to insure favorable circumstances for study. Out-of-town patients avail themselves of a near-by hotel. A couple sincerely interested in childbearing will not offer ob-

jection to a plan so easily executed and one devoid of artificial and distasteful features. I have gained acquiescence to this simplified program in every instance except one and the husband who objected related at my office the following day a history of double epididymitis just before marriage. At the appointed hour I have ready on a table, beside the examining table, a warm microscope and a warmed tray, which I call the sterility tray, which contains the following articles: clean glass slides with blank labels, cover glasses, litmus paper, and numerous pipettes of various types and sizes including Skene's pipettes, some of which I have modified so that they may be most useful for obtaining specimens from the cervical canal, or for injection in the tube patency test which I shall describe later.

The patient, having arrived at the appointed hour, is prepared, with loosened corset, in the usual dorsal position. The vagina and cervix are exposed with a warm speculum introduced without lubricant, for obvious reasons. The examination should proceed at once. Under normal conditions, the examiner will find a pool in the vaginal sulcus varying greatly in amount, but averaging about a dram, consisting of seminal elements mixed with vaginal secretion. The pool is thin in density; slightly alkaline in reaction; and when transferred to the microscope shows a field swarming with vigorously moving spermatozoa amid vaginal and seminal debris. To avoid errors of contamination, the vagina is now dried and if mucus is protruding from the cervical os a specimen is taken for examination. This mucus is then removed and specimens of mucus taken from the cervical canal up to the internal os. Some difficulty may be experienced in obtaining tenacious mucus, but with patience and a trial of different pipettes, avoiding roughness that causes oozing, specimens will be obtained which, when spread under a cover glass, will show from four to sixteen active sperm cells to a field. Cells which have made this progress to the cervical canal are almost invariably normal as to morphology and mobility showing Nature's simple plan that only the fittest shall survive to perpetuate the species. Vaginal elements are sometimes found in the cervical canal. In connection with this, I wish to say that a peristaltic action of the uterus as a phenomenon of the orgasm seems proved, for, in experimental study I have found spermatozoa with vaginal epithelium, and other elements well within the cervical canal immediately after intercourse. Thus may be explained the contradictory experience as to the value of the douche as a contraceptive measure.

DEFECTIVE INSEMINATION

The absence of the male element in the vagina or the presence of seminal fluid but with few or no spermatozoa, establishes at once the male responsibility and the problem is transferred to the genitourinary surgeon. Again, if the examination demonstrates a normal pool retention with the usual sperm content but without motility or with few active cells; or with sluggish activity with decreased or absent motility in the cervical canal only two conclusions are possible; either the male element has been deficient from the first or, having been normal, has been adversely affected by the vaginal secretion. To determine which is the correct hypothesis our investigation must be temporarily diverted to the husband. A specimen obtained directly in one of the methods described previously before this society,1 is examined after the lapse of forty minutes just mentioned. Gross errors will result if this technic is careless; but if correctly carried out, one may reach conclusions with confidence. It was some time before I acquired the courage to ascribe sterility to male specimens of partial deficiency. I have nothing of greater importance to state tonight than that while a specimen with some motility is theoretically capable of impregnation, experience has shown that pregnancy does not occur if the male product is definitely weak. Many couples come to me with the statement that the husband has been examined and is "all right." In checking up this finding I am convinced that the average examination proceeds no further than the demonstration of some active cells. One husband said he had made a diagnosis himself of normality, for, after the doctor put the specimen under the microscope, he plainly saw things wiggling. Cooperation of an associate in genitourinary diseases will invariably show vital anatomic or functional troubles in the men having decreased activity of spermatozoa and there is no reason to modify the statement, made some years ago, that when our investigation has proceeded thus far-practically after two visits-fully fifty to sixty per cent of the cases will be eliminated as gynecologic problems.

If the male specimen obtained directly proves fertile and a second insemination test shows its vitality to be again destroyed, the adverse action is shown to be definitely in the vagina. In our experience this has always been due to hyperacidity though others have ascribed it to a toxicity of bacterial origin. The latter is a reasonable assumption and worth investigation. Hyperacidity may be assumed, if the vaginal pool is found definitely acid. Spermatozoa soon die in such a medium. Hyperacidity is a relative term in sterility and we do not ascribe to it the importance which was formerly noted. This test, however, affords a means of fixing its responsibility—definite and destructive in occasional instances. The spectacular cures sometimes following alkaline douches are thus accounted for, but the reverse does not hold good that hyperacidity is ruled out by the haphazard use of bicarbonate of soda or sodium hydrate douches, for marked vaginal acidity is not so easily corrected.

When in cases of the second group a normal semen is demonstrated

and the patient is free from any suspicion of pelvic inflammatory changes, one is justified in studying and correcting these minor lesions as a possible etiologic factor. No discussion regarding treatment shall be given here. Flexions and other anatomic signs should encourage investigation of delayed development and appropriate treatment. There are more cases of this type than is commonly thought. Correcting a version may clear up an endometritis or some other unfavorable condition. Cures will certainly follow in a certain proportion of cases from the use of a pessary; or from dilatation of the cervix, with or without stem insertion, and for reasons that are not always definitely explainable.

When the minor lesion of a patient in group two is corrected for several months without pregnancy occurring, the patient automatically belongs in group three, that is, patients without demonstrable pathology. When spermatozoa are traced in normal activity to the internal os we may assume that in these patients sterility is due either to deficient ovum production, or a blockade in the fallopian tubes, or, possibly, a change in the endometrium which prohibits growth. Very frequently indeed, upon the conclusion of the test at the second visit of the patients, who formerly were particularly puzzling because so normal, the etiology of the sterility may be fixed within these narrow limits. Delay in development; illness or hard work during the years of puberty; absence of normal instincts; disturbed function, such as scant menses or intermittent amenorrhea may strongly point to endocrine deficiency, involving the ovary, but not conclusively proving absence of ovulation. The study of this relation offers an opportunity for valuable research.

In by far the largest percentage of cases in this group the lesion is in the fallopian tubes. It seems we have been grievously slow when studying these cases to recognize that the tubes are frequently sealed by gradual processes, other than gonorrheal or tuberculous, and without marked symptoms or palpable pathology. Probably each of you in performing a laparotomy has often had the experience of finding sealed tubes in patients who gave not the slightest suspicion of it either in the history or the vaginal examination. Many of these obstructions probably develop from the action of the colon bacillus.

Appreciating the difficulty of recognizing this important lesion, I proposed some years ago to prove the patency of the tubes by radiography after collargol injections into the tubes.² This proved impractical, but suggestive. If we cannot claim that this problem is already solved, at least the study is well on the way. Rubin has proposed, and is using, a most ingenious method of proving tubal patency by demonstrating the injection of oxygen into the peritoneal cavity

through the uterus. I have for some time been developing another method, similar in principle, by demonstrating the injection of sterile normal saline solution through the tubes by carefully measuring the amount injected in excess of the 8 or 10 minims known to be the capacity of the uterine cavity. In other words, by experimental injection through the uterus, just before laparotomy, we found that the reception of 16 minims of solution means escape through a tube. The possibility of such a test was first suggested to me in attempting artificial impregnation. In some patients the injection proceeded easily with definite peritoneal reaction. In others there was an equally definite limitation to the injection and almost no progress was made. After a little experience, the ease with which the solution is injected is as significant as the amount introduced.

When by a process of exclusion, as represented by the insemination test, the cause of sterility is practically limited to the tube or ovary, the history of associated findings will usually make the differential diagnosis possible. In my opinion the necessity of proving the patency of the tubes will be exceptional. I limit the test to the most carefully selected patients who give no definite past history of pelvic inflammatory disease and when two pelvic examinations have caused no reaction.

TECHNIC OF INJECTION TEST

The injection test has simplicity to commend it. Freshly made normal saline solution is used and is boiled immediately before injection. Pipettes with the Skene curved tip of varying sizes, are used. The pipette is chosen which snugly fits the individual patient and is carefully sterilized. Sixteen minims of the solution are taken up in the pipette. The patient with emptied bladder and removed corset is placed in a perfect knee chest position and the cervix exposed with the Sims speculum. The vagina is wiped dry and the external os sponged free of mucus. Mucopurulent material in the canal is considered a contraindication to the test. Strips of dry paper are placed in the most dependent part of the vagina to aid the eye in detecting escape of the solution around the pipette. The pipette is now snugly engaged in the canal of the ungrasped cervix. Gentle pressure is then exerted upon the rubber bulb, tightly tied to avoid leak, and a headlight fixed upon the pipette and cervix. Moderate pressure is steadily exerted and the patient instructed to take a deep breath. It is surprising how readily the saline disappears if either tube be open. If they are closed no headway will be made; the pipette will be removed with most of the saline still in it, or leakage will occur about the pipette which, even if undetected by the eye, will soak the tell-tale strips of paper. No immediate discomfort to the patient follows the successful injection, but about three hours later a definite peritoneal reaction occurs with some pain in the nature of a colic. Patients are told to expect this and to stay in bed over the anticipated period. Larger amounts of saline are unnecessary and are cautioned against because of the more painful reaction.

That this test, applied for purposes of diagnosis, may possibly also be responsible for the opening of a sealed tube, was demonstrated in the patients, previously referred to, who were laparotomized after an injection into the tubes. I am of the opinion that, except in vaginal or cervical disease, when artificial impregnation succeeds it does so because occasionally a blocked tube is thus opened. This is the only

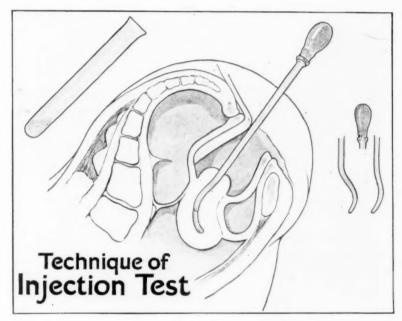
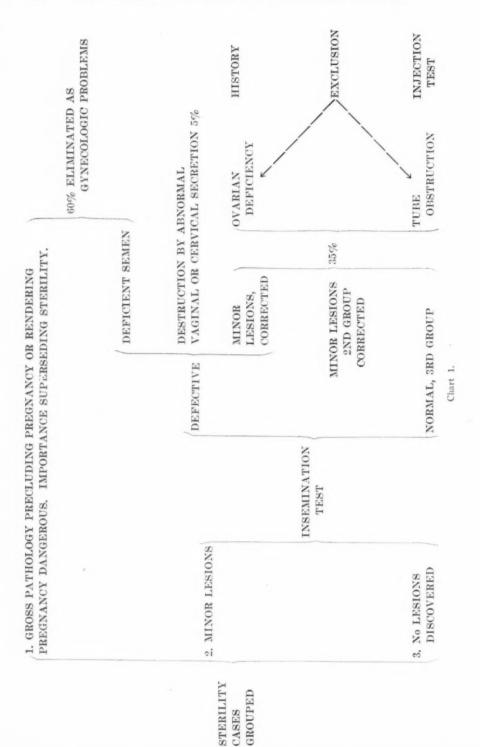


Fig. 2.

rational explanation and for this reason I have practically abandoned attempts at artificial impregnation except to facilitate the passage of spermatozoa through the cervical canal. If my opinion is correct, normal saline is certainly safer for purposes of injection than is seminal fluid. The possibility of carrying infection to the peritoneal cavity by this method is recognized if the technic be careless and the cases improperly chosen. If, however, we have confidence in the peritoneum allowing us to close the abdomen without drainage after hysterectomy, incising across the cervix or vaginal cuff, and if we contaminate the peritoneum with the contents of tubes with impunity, why should our confidence in the peritoneum be abandoned in the instance of this lesser danger?



CONCLUSIONS

While one's results in treating sterility may not be brilliant, the prognosis would be greatly improved if patients sought competent study earlier.

The indiscriminate use of the curette in the treatment of sterility is to be emphatically and unconditionally condemned. In the hands of an expert it may be condoned as an instrument of diagnosis; rarely for treatment.

Further study confirms that the husband shares equally the responsibility for sterility. Casual reports of male virility are not to be accepted. Sterility may be attributed to partial defects of the semen. These cases offer an excellent prognosis.

A simplified method of studying sterility, original³ only in its application, is presented as outlined in the accompanying chart. It eliminates most of the unpleasant features of previous methods and enables a quick and definite diagnosis of the inhibiting lesion.

Finally, a technic, for proving the patency of the fallopian tubes in carefully selected cases, is elaborated, which is so simple as to be available to any gynecologist of ordinary training and equipment.

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 Hühner, M.: Practical Treatise on Disorders of Sexual Functions in the Male and Female, 1916.

(For discussion, see page 440.)

A PRELIMINARY REPORT ON THE TREATMENT OF SYPHILIS COMPLICATING PREGNANCY*

BY ALFRED C. BECK, M.D., BROOKLYN, N. Y.

From the Department of Gynecology and Obstetrics of The Long Island College Hospital.

S INCE the establishment of our prenatal clinic in 1912, much consideration has been given to the subject of syphilis in pregnant women. At first very little was accomplished because our lack of laboratory facilities compelled us to rely solely upon the history and physical findings for diagnosis. Only rarely was it possible to obtain a history of the common symptoms of this complication. Physical examination likewise was of very little help and a positive diagnosis seldom was made except in those patients whose previous pregnancies ended in miscarriages or premature stillbirths.

Five years ago access to a Wassermann laboratory was obtained and the serological test of each patient was added to our routine. About 6 per cent of the applicants to our clinic showed a 3-plus or a 4-plus reaction. The addition of these facilities was a great aid in the cases that applied for care early in their pregnancies. As the majority of our patients came to us in the last trimester, satisfactory results were exceptional since insufficient funds forced us to depend upon mercury alone in the treatment of this condition.

In 1919 funds for the administration of salvarsan were obtained and it was possible to give our syphilitic patients intensive treatment. While the number of cases seen since the establishment of the free salvarsan clinic is small, the results have been so striking that your chairman has asked me to present them for your consideration. Thirty-two patients have been followed up after confinement. The accompanying Summary of Cases gives brief details of each case.

DIAGNOSIS

The history of symptoms and physical examination were of little service in making the diagnosis. Of the thirty-two patients only two gave the history of a rash. Two had sores on the vulva. One showed mucous patches and another had tabes. History of symptoms and physical examination aided in the diagnosis of only six, or 18.7 per cent of the total cases.

The history of the previous pregnancies was more valuable. Six-

^{*}Read at a meeting of the Section on Obstetrics and Gynecology of the New York Academy of Medicine, April 26, 1921.

teen, or 61.5 per cent, of the patients who had previous pregnancies, gave a history of miscarriages, stillbirths or living syphilitic infants and the characteristic tendency to carry each pregnancy further than the preceding ones was noted in seven. Eleven never had a miscarriage or stillbirth but always gave birth to living infants. After deducting from these eleven cases, three (8, 13, and 14) in which the diagnosis is open to question on account of its having been based solely upon Wassermann findings, there remain eight, or 34.7 per cent, of the multiparae who carried all of their pregnancies to viability and gave birth to living infants of whom the majority probably were syphilitic. This high percentage of living infants born to untreated syphilitic mothers emphasizes the importance of disregarding a negative previous pregnancy history. It also shows the value of the routine Wassermann as well as the need for a microscopic examination of every placenta for syphilis. If private patients object to the examination of their blood during pregnancy the placenta at least may be examined in order that these unsuspected cases of congenital syphilis may be recognized and treated early.

The routine Wassermann test has been of great assistance. In our earlier work a single test was made. Lately we have had all positive reactions checked in the same or a different laboratory. All of the cases included in this report gave a strongly positive reaction. The Wassermann during pregnancy at times has been confusing and in several instances patients who have had no treatment showed a 3-plus or a 4-plus reaction during pregnancy and a negative one after delivery. This experience has led me to be extremely careful in making a diagnosis upon Wassermann evidence alone. The seven cases marked with an asterisk in the accompanying Summary of Cases were diagnosed as syphilis solely upon Wassermann findings during pregnancy. Four of these occurred early in the series and as a result only one blood examination was made. The remaining three had repeated positive reactions. While it is possible that some of the seven patients were not syphilitic, it is probable that most of them were infected. My reason for including these questionable cases in this report is the fact that they introduce the question of the justifiability of intensive antisyphilitic treatment when the only evidence of syphilis is the presence of a positive Wassermann during pregnancy.

TREATMENT

The plan of treatment followed in this series consisted of the administration of salvarsan and mercury as soon as the diagnosis was made. Four decigrams of salvarsan and one mercury injection were given weekly for six weeks. In addition the mercury was continued for six weeks more. If after these six salvarsan and twelve mercury in-

jections the Wassermann test remained positive, a second similar course was repeated. One hundred and fifty-one intravenous salvarsan injections were given at various stages of pregnancy and in only one case was it necessary to discontinue the use of this drug because of unfavorable effects. The urine after treatment showed more than the occasional trace of albumin which is found during pregnancy. In those cases which were first seen too late in pregnancy to permit of the use of a full course of the drug, a few injections during the latter weeks were of great benefit. Following delivery, treatment was resumed as soon as the patients were able to return to the clinic.

All infants were referred to the department of pediatrics immediately after delivery. They received daily mercury rubs for two months. If the Wassermann test was negative one month later no further treatment was given unless evidence of syphilis appeared. The infants of the patients who had not been given sufficient prenatal treatment and those in whom evidence of congenital syphilis was observed were treated more intensively. In addition to the daily rubs, 0.5 c.c. of mercury oxycyanate was administered weekly for six weeks at the end of which time intravenous salvarsan was commenced.

END RESULTS

In considering the end results I have divided the cases into three groups according to the amount of salvarsan administered before delivery. Group 1 contains four cases that received no salvarsan.

END RESULTS OF TREATMENT IN INFANTS OF 32 SYPHILITIC MOTHERS



Fig. 1.—4 cases. No salvarsan before delivery.



Fig. 2.—11 cases. 1-to-3 salvarsans before delivery.



Fig. 3.—17 cases. 5+ salvarsans before delivery.

Group 2 consists of eleven cases that had from one to three injections of this drug. Group 3 is made up of seventeen that had five or more injections. A graphic representation of the results in each group is shown in Figs. 1, 2, and 3.

No salvarsan was given in Cases 2, 18, 31 and 32. All of these gave birth to living children. Two (2 and 31) had previous stillbirths and the birth of living syphilitic infants in their cases was not unexpected.

Case 32 in all probability contracted syphilis after her previous child was born. According to tradition, the birth of a living child in this case is unusual. However, as 34.7 per cent of our syphilitic multiparae had living infants without previous miscarriages or stillbirths, we do not look upon this as a very uncommon course. At or soon after birth three of the four infants in this group showed signs of congenital syphilis. They have been under observation 4, 6½ and 7 months, respectively, and have not responded to treatment as well as the syphilitic infants whose mothers had salvarsan before delivery. The fourth child has been observed for six months and has not revealed any of the stigmata of lues. Its mother's Wassermann was strongly positive before and after delivery. (See Fig. 1.)

One to three injections of salvarsan were given to eleven cases. Ten of these gave birth to living infants one of which died on the third day and another, a premature, died from pneumonia (?) two months after birth. Five of the remaining eight have been under observation $\frac{1}{2}$, 3, $\frac{4}{2}$, 6 and 16 months and have never shown any evidence of syphilis. The other three of the living infants in this group had congenital syphilis. They did well under treatment and had negative Wassermanns at 11, 11, and 13 months. (See Fig. 2.)

Five or more injections of salvarsan were given in seventeen cases. Fifteen gave birth to living infants and two miscarried at the fifth month. Fourteen of these infants have never shown any evidence of syphilis. (They have been under observation 1, 1, 1, 2, 2, $4\frac{1}{2}$, 6, 6, 8, 10, 11, 12, 15 and 18 months.) The one syphilitic child in this group is doing well under treatment. (See Fig. 3.)

Fourteen living and apparently nonsyphilitic infants from seventeen syphilitic mothers is a result that could not have been obtained even under the best circumstances when we relied upon mercury alone in the treatment of this condition. Even in the last weeks of pregnancy, when insufficient time remained for the administration of a full course of salvarsan, a few injections of this drug proved beneficial and the pediatricians who followed our cases observed that the luctic infants born to mothers who had these small amounts of salvarsan before delivery responded more readily than usual to antisyphilitic treatment.

SUMMARY

The history and physical examination were of little value in making the diagnosis of syphilis. They aided in only six, or 18.7 per cent of the cases.

The history of the previous pregnancies was helpful in sixteen, or 61.5 per cent of the multiparae.

Eight unquestionably syphilitic multiparae carried all of their pre-

vious pregnancies to the period of viability and gave birth to living infants, 34.7 per cent.

This high percentage of living infants born to untreated syphilitic mothers emphasizes the need for the routine prenatal Wassermann and the microscopic examination of every placenta.

If private patients object to the Wassermann test the placenta at least may be examined in order that early treatment in latent cases may be inaugurated.

The routine Wassermann test is a most valuable aid to prenatal work. This test, however, at times is misleading, for a positive reaction during pregnancy occasionally becomes negative after delivery, even though no treatment has been given.

Salvarsan may be given at any stage of pregnancy and its value is indicated by these results.

Eleven cases that received one to three injections gave birth to five living nonsyphilitic infants and three infected ones that have done remarkably well under treatment.

Seventeen cases that received five or more injections were delivered of fourteen nonsyphilitic and one syphilitic infant and the infection in the latter has been easily controlled by treatment.

I wish to acknowledge my indebtedness to the Genitourinary and Pediatrie Departments of the hospital for their splendid cooperation in the care of these patients.

SUMMARY OF CASES

Case 1.—U. J., age twenty-three, black, one five-months' miscarriage, two premature deliveries at 7 and 7½ months, no symptoms or signs of syphilis other than pregnancy history. Wassermann 4+, repeated 4+. Six injections of salvarsan and seven of mercury before delivery. Wassermann, after delivery, negative. Six injections of salvarsan and five of mercury after delivery.

Child never showed any evidence of syphilis. Under observation 1½ years, at the end of which time its Wassermann was negative.

Case 2.—M. S., age twenty-seven, white, two miscarriages at second and third months, one premature labor at 7½ months, one stillbirth. No symptoms or signs of syphilis, other then pregnancy history. Wassermann 4+, before delivery; no treatment. Child alive at birth; later developed a rash, a large spleen and suffered from inanition. At the end of six weeks its Wassermann was 4+. Intensive treatment given. Wassermann negative at the end of 5½ months. Last seen at the age of 6½ months, when it was doing well.

Case 3.—M. H., age thirty-three, white, had three miscarriages and seven living infants. No symptoms or signs of syphilis. Wassermann 4+ before delivery. Six injections of salvarsan and five of mercury before delivery. Wassermann negative after delivery. Child born alive, and has never shown any signs of syphilis. Its Wassermann at 5½ months was negative. This infant has been under observation 10 months.

Case 4.—M. M., age twenty-nine, white, one previous living child. Wassermann 4+ before delivery. Repeated Wassermann 4+. Six injections of salvarsan and

of mercury before delivery. Wassermann positive, after delivery. Child alive at birth. Its Wassermann negative at sixth week. No evidence of syphilis developed during the six months it has been under observation.

Case 5.—L. B., white, primipara. No symptoms or signs of syphilis. Wassermann 4+. Six salvarsan and mercury injections before delivery. Child alive and has shown no signs of syphilis during the 14 months that it has been under observation.

Case 6.—C. S., twenty-six, white, one living child. History of rash. Wassermann 3+ before delivery. Repeated Wassermann 3+. Two salvarsan and three mercury injections before delivery. Wassermann 3+ after delivery. Four salvarsan and mercury injections after delivery. Child alive, no evidence of syphilis at any time. Wassermann negative at sixteen months.

Case 7.—P. S., seventeen, white, primipara, a prostitute. No symptoms or signs of syphilis other than 4+ Wassermann. Five salvarsan and mercury injections before delivery; one salvarsan and twelve mercury injections after delivery. Child born alive and never showed any signs of syphilis. Age when last seen eight months.

Case 8.—O. B., twenty-three, white, one living child, no symptoms or signs of syphilis other than 4+ Wassermann. Six salvarsan and nine mercury injections before delivery. Child born alive and never showed any evidence of syphilis. Last seen at the age of eleven months.

Case 9.—E. M., age twenty-one, white, primipara, history of mucous patches, Wassermann 3+. Three salvarsan and two mercury injections before delivery. Five salvarsan and sixteen mercury injections after delivery. Premature child, 2200 grams, born alive but died at the age of two months in another hospital. Diagnosis. "Pneumonia."

Case 10.—A. H., age thirty, white, two living, syphilitic children. No symptoms or signs of syphilis. Wassermann 3+, Repeat 3+. Six salvarsan and five mercury injections before delivery. Wassermann 2+, after delivery. Nineteen mercury injections after delivery. Child born alive and never showed any evidence of syphilis. Its Wassermann was negative at 15 months.

CASE 11.—A. B., thirty-four, white. One previous living child which died at three weeks. Wassermann 4+. Repeat 4+. No other evidence of syphilis. One salvarsan and two mercury injections before delivery. Wassermann 4+ after delivery. Five salvarsan and nineteen mercury injections after delivery. Child born alive, later developed rash, enlarged spleen and glands. Its Wassermann was negative at the age of 13 months.

CASE 12.—L. S., seventeen, white, primipara history of sore on vulva and prolonged sore throat. Wassermann 4+. Repeat Wassermann 4+. Twenty salvarsan and eighteen mercury injections before delivery. Wassermann 3+ after delivery. One salvarsan and nineteen mercury injections after delivery. Child born alive. Its Wassermann was negative at 3½ months. No evidence of syphilis was observed during the 4½ months it was under observation.

Case 13.—E. W., twenty-nine, black, two living children. Wassermann 2+. Repeat 4+. No other evidence of syphilis. One salvarsan and one mercury injection before delivery. Five salvarsan and seven mercury injections after delivery. Child born alive, under observation 6 months. Never showed any signs of syphilis.

Case 14.—M. F., thirty-four, white, one misearriage 6½ months, condylomata of vulva. Wassermann 2+. Repeat 4+. One salvarsan and two mercury injections before delivery. Child born alive and developed rash, snuffles and glands. Under observation 11 months. Doing well.

CASE 15.—M. D., twenty-three, white, one 8-months' stillbirth, one living infant which died at the 6th week. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Six salvarsan and nine mercury injections before delivery. Wassermann 3+ after delivery. Two mercury injections after delivery. Child born alive; later developed glands. Under observation 3 months. Doing well.

Case 16.—E. T., twenty-one, black, one miscarriage, two stillbirths at 8 and 10 months. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Three salvarsan and eight mercury injections before delivery. Wassermann 4+ after delivery. Six salvarsan and nine mercury injections after delivery. Premature child, 2300 grams, born alive. Wassermann 2+ at 2 months. Negative at 11 months. Under observation 19 months. Doing well.

CASE 17.—E. T., twenty-two, black, same as Case 16. Wassermann 1+. Six salvarsan and twenty-two mercury injections before delivery. Child born alive. Under observation 1 month. No evidence of syphilis.

Case 18.—F. S., twenty-six, white, one living child. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Two mercury injections before delivery. Wassermann 4+ after delivery. Child born alive. Never showed any signs of syphilis. Its Wassermann was negative at six months.

Case 19.—A. M., thirty, black, primipara. Wassermann 4+. Repeat 3+. No other evidence of syphilis. Six salvarsan and fourteen mercury injections before delivery. Wassermann 4+ after delivery. Six mercury injections after delivery. Child born alive. No evidence of syphilis. Wassermann negative at 6 months.

Case 20.—A. L., thirty-five, white, one 8-months' stillbirth. Four dead children. Four living syphilitie children. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Two salvarsan and one mercury injection before delivery. Wassermann 4+ after delivery. Child born alive. Died on the third day. Syphilis.

Case 21.—E. M., twenty-nine, white, one living *syphilitic* child. Wassermann 4+. Repeat 4+. Patient has tabes dorsalis. Eight salvarsan and seventeen mercury injections before delivery. Wassermann 4+, after delivery. Four salvarsan and three mercury injections after delivery. Child born alive, Under observation 2 months. No evidence of syphilis.

Case 22.—K. C., thirty-four, white, one stillbirth and four living children. Wassermann 4+. Repeat 4+. No other evidence of syphilis. One salvarsan and two mercury injections before delivery. Child full term, born dead. Syphilis.

Case 23.—T. S., thirty-two, white, seven living children. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Three salvarsan and one mercury injections before delivery. Wassermann 4+ after delivery. Child born alive. Under observation 3 months. No evidence of syphilis.

Case 24.—C. R., twenty-five, white, five living children. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Six salvarsan and seventeen mercury injections before delivery. Wassermann negative after delivery. Child born alive. Under observation one month. No evidence of syphilis. Wassermann negative.

Case 25.—M. M., twenty, black, two miscarriages. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Six salvarsan and nine mercury injections before delivery. Wassermann 4+ after delivery. Child stillborn at the fifth month. Syphilis.

Case 26.—G. P., twenty-six, white, two dead infants. Wassermann 4+. Repeat 4+. No other evidence of syphilis. One salvarsan and one mercury injection before delivery. Child born alive at term. Under observation 2 weeks. No evidence of syphilis.

Case 27.—O. N., eighteen, white, primipara. Wassermann 4+. Repeat 4+. No other evidence of syphilis. Two salvarsan and two mercury injections before delivery. Five salvarsan and nine mercury injections after delivery. Child born alive. Wassermann negative for 4½ months. Under observation 4½ months. No evidence of syphilis.

CASE 28.—M. J., twenty-two, white, one living 8-months' premature child. Died on eighth day. Wassermann 4+. Repeat 3+. History of a rash. Ten salvarsan and three mercury injections before delivery. Wassermann 3+ after delivery. Child born alive. Under observation two months. No evidence of syphilis. Wassermann negative.

CASE 29.—S. S., twenty-one, white, one 8-months' premature infant which died. One full term living infant. Wassermann 2+. Repeat 4+. No other evidence of syphilis. Six salvarsan and seven mercury injections before delivery. Wassermann 4+ after delivery. Four injections of salvarsan after delivery. Child stillborn at the 5th month. Syphilis.

Case 30.—L. R., thirty-two, white, one stillbirth. Wassermann 3+. Repeat 4+. No other evidence of syphilis. Sixteen salvarsan and fifteen mercury injections before delivery. Child born alive. Wassermann negative at 12 months. Under observation 12 months. No evidence of syphilis.

Case 31.—V. F., twenty-eight, white, one stillbirth at 8 months. Wassermann 4+. Repeat 4+. No other evidence of syphilis. No treatment before delivery. Child born alive. Developed snuffles, enlarged spleen, liver, glands, etc. Under observation 4 months. Doing well.

Case 32.—F. C., twenty-four, white, one living child. Wassermann 4+. No treatment before delivery. Child born alive. Wassermann 4+ at the 3rd month. Under observation 7 months. Epiphysitis of femur. Doing well.

20 LIVINGSTON STREET.

(For discussion, see page 448.)

Society Transactions

AMERICAN GYNECOLOGICAL SOCIETY. FORTY-SIXTH ANNUAL MEETING HELD IN SWAMPSCOTT, MASS., JUNE 2, 3, AND 4, 1921

(Continued from September issue)

DR. HIRAM N. VINEBERG, of New York, read a paper entitled Vaginal Supracervical Hysterectomy with Interposition of the Cervical Stump for Cystocele and Procidentia Associated with Enlargement of the Uterus. (For original article see page 368.)

DISCUSSION

DR. THOMAS J. WATKINS, CHICAGO, ILL.—Personally, I feel the term "interposition" is an unfortunate one, and prefer the term "transposition" of the uterus and bladder. It is a bladder operation. To specialists like Dr. Vineberg it makes no difference whether you call it interposition or transposition, but to others the term is misleading.

Dr. Vineberg did not tell us the size of "the uterus that is too large." In our earlier cases we extended this operation in a research way, to determine how large a uterus could be successfully transposed. We transposed some that were too large. What happened? The fundus protruded and a wedge-shaped piece of the fundus had to be taken off later in some 4 or 5 cases. A uterus transposed that is too large never gives trouble afterwards because of remaining large. It always atrophies. The increased amount of atrophy may permit the fundus to protrude.

The most important feature in this paper is the question of infection. The dangers of infection vary with the amount of operative work done. One can transpose the uterus with very little danger of infection, but with excision of parts of the body of the uterus the danger of infection is much increased. The danger of infection is so much increased that if I have to excise the fundus and transpose the cervix, I usually make a supravaginal hysterectomy from above and transpose the cervix from below. The vagina is always a septic field, irrespective of cleansing and antiseptics. The wound remains in close contact with the septic area. It does not take any extensive bacteriologic investigation to prove this contention. Gauze left in the so-called sterilized vagina soon becomes offensive. Serum and blood after operation in contact with the vagina soon become infected.

The modified transposition operation of suture of a portion of the broad ligaments after excision of the anterior lip of the cervix, thus shortening and bringing the severed portion of the broad ligaments together and doubling the cervix upon itself, has given excellent results in selected cases. The uterus is thus left high in the pelvis supported by strong buttress. If supravaginal hysterectomy and amputation of the cervix is made per vaginam the entire mucosa of the cervix should be excised to save time and a possible stenosis.

DR. GEORGE GRAY WARD, JR., New York City.—This operation described by Dr. Vineberg is one that has interested me for some time. I have done it on

several occasions in past years without being aware as to who was the originator of the procedure, and I find it very satisfactory.

In looking up the literature of the subject I found that in 1909 Löwit and Rieck published (Monatschr, f. Geb. u. Gyn., xxx, 724) a report of an operation which, as far as I can see, has the same principle, and therefore I presume the eredit for this procedure more or less belongs to them. They state in their report that the corpus uteri is removed and the cervix uteri sewed into the vesicovaginal septum to support the cystocele. That is the principle of the operation. In 1911 I understand Pfannenstiel, Cohn, and Franque also practiced this method. We all see cases with a large cystocele, with diseased cervix, and a uterus that is not suitable to leave or to interpose, and we occasionally see a uterus in such cases with a large growth which makes it difficult to do this operation through the vagina alone. In these cases we have been in the habit of doing an amputation of the cervix and separation of the bladder from below, with repair of the pelvic floor, and then opening the abdomen for the rest of the procedure, and my associate, Dr. Farrar has worked out a technic for this type of case which has proved very satisfactory and the results have been all one could desire as far as we have been able to follow them. A description of the technic will appear in the AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY.*

As to the matter of infection to which Dr. Watkins has referred, I think he is correct when he says that the amount of infection is proportionate to the amount of manipulation. That is practically true in all vaginal procedures that are extensive. We have been in the habit of using a rubber tissue drain inserted in the line of incision on the anterior vaginal wall, which is fastened to a piece of silk or linen, and is removed on the morning of the fifth day as a rule. I believe this has prevented the formation of a hematoma and possibly infection in many of these cases. We use it in the transposition operation or any procedure where there is extensive separation of the bladder from the anterior wall of the vagina.

DR. G. BROWN MILLER, WASHINGTON, D. C.—Those of us who do cystoscopic work in connection with gynecology will be interested in inquiring of Dr. Vineberg what percentage of his cases had bladder symptoms subsequent to this operation. I have had the opportunity to observe some cases in which the interposition operation has been done, not as Dr. Vineberg described it, but in which the whole uterus has been interposed between the bladder and anterior vaginal wall, and some of them have had marked bladder symptoms. In one case that had an intractable bladder condition I found a small fibroid tumor pushing up the under surface of the bladder which had developed since the operation was done. We know that some of the worst results from the Wertheim and similar operations, such as complete hysterectomy, in which the bladder has been traumatized by being separated from the uterus, have been due to infections of the bladder and as trauma of the bladder predisposes to this condition, as a rule, I believe that so far as we can, in our operations upon the vagina and uterus, we should inflict as little traumatism as possible. Dr. Vineberg's statistics include a number of infections and vesicovaginal fistulae, and I would be surprised if some of his cases did not have cystitis following operation and perhaps conditions of the bladder that took a considerable time to overcome if they were overcome at all.

DR. J. RIDDLE GOFFE, New YORK CITY.—About 25 years ago I revived interest in and called attention especially to the condition which the paper claims to relieve. Up to that time the Emmet operation was the only one in vogue, and having had failures from it, as we all had, I studied the situation and evolved an entirely original method. I do not need to go into detail before this Society

^{*}See page 403 of this issue.

in explaining the procedure. The one feature that was absolutely original was the removal of the uterus and the substitution of the broad ligaments as a basis of support by stitching them together across the pelvis. This relieves the situation and gives a plane of tissue for support that answers perfectly the condition we have when a small uterus is there. It therefore seems to me preferable to the modification just presented by Dr. Vineberg in cases of large septic uteri. But what I desire especially to direct attention to this morning is the fact that Dr. Charles Mayo has adopted this original procedure of mine, combined it with an equally original procedure of another member of our Society, Dr. Watkins, and with elaborate illustrations published it to the profession as an original procedure of his own. It is therefore becoming known among general surgeons as the "Mayo operation." This is indignity enough, but when we find members of our own Society announcing on the bulletins of their daily clinics the Mayo operation for procidentia, what can we say of their loyalty to their specialty and to their fellow members of this Society? When reproached for this injustice, the reply of one of our members has been, "Well, you know Mayo is a name to conjure with." Is it honest, is it fair to employ procedures invented, employed and published to the profession by our own members and apply to them the name of a general surgeon because for such "it is a name to conjure with," and may gain his approbation?

As I have remarked, Dr. Mayo gives credit to nobody for it, and yet there are two members of this Society who are responsible for the whole procedure. It should have the name of Watkins or Goffe, or both, rather than that of any other individual.

DR. JOHN A. McGLINN, Philadelphia, Pennsylvania.—The procedure outlined by Dr. Watkins and the one outlined by Dr. Goffe are sufficient to cure cystocele and prolapse of the uterus. I find in my work there are certain types of uteri which are not proper to interpose, and in these cases I do not see any reason why the operation should be complicated by leaving any part of the uterus to act as a dangerous point for infection or for possible malignancy, and I usually take out the uterus, which is much easier than to do a subtotal hysterectomy through the vagina and interpose the broad ligaments between the bladder and vagina. I do not refer to this operation as the Mayo operation.

Some years ago I saw Dr. Goffe do his operation, and one day I did the operation according to Dr. Goffe's technic long before the so-called Mayo operation was published. I placed my stitches in such a way that I found I did the operation which was eventually called the Mayo operation. I refer to the operation entirely in my work as Dr. Goffe's operation, because I think Dr. Goffe is the originator of it, but I have a slightly different way of doing the procedure from what Dr. Goffe advocated. Why we should leave behind a cervix which may undergone degeneration, which may give rise to an intractable leucorrhea, which may act as a point of infection, when total hysterectomy through the vagina is much easier than the other procedure, I do not know. This operation is sufficient to cure as I have had experience with it recently. A general surgeon not far from the city in which I live does not believe that the gynecologist should exist as such, and so far as obstetric cases are concerned, the only obstetric procedure which he does is a cesarean section done at variable periods during pregnancy. This gentleman attempted to cure a prolapse of the uterus recently by doing a total abdominal hysterectomy. He cured the prolapse of the uterus because there was no uterus left behind to prolapse, but nothing was done on the prolapsed vaginal wall, so that when the case came to me she had a total prolapse of the vagina. I resorted to the vaginal procedure, opening up and isolating what was left of the broad ligaments and suturing them together according to the method of Dr. Goffe, and then doing extensive plastic work sufficient to cure the cystocele and rectocele. So long as this operation of complete hysterectomy by vagina with the technic evolved by Dr. Goffe is sufficient to bring about a cure, why complicate the operation by other procedures, by leaving behind a part of the uterus?

DR. ROBERT L. DICKINSON, BROOKLYN, NEW YORK.—Having done between 40 and 50 Goffe operations for prolapse of the uterus before the Mayos did it, and having put a single suture through the broad ligaments as a modification of Goffe's operation, and published it long before the Mayos were using it, I desire to back up what Dr. Goffe has said. A single step in the so-called Mayo procedure is the very desirable provision of putting on four clamps, which brings within the reach of the surgeon the broad ligaments to suture more readily and possibly prevents slipping away of the upper ends of the broad ligaments which in unskillful hands might provoke secondary hemorrhage.

I also have tried the modifications spoken of by Dr. Vineberg, and it nearly doubles the length of the operation to leave in the cervix and have it perhaps infected at both ends. In other words, you can build up the broad ligaments amply

and have a sufficient bridge with which to hold up the bladder.

Dr. Goffe has not spoken of one of the most important items in his operation, and that is, the front bridge of broad ligaments, or better the unrelaxed broad ligaments back of the top of such a bridge, which constitutes a nearly perfect support for even the worst forms of cystocele, and these worst forms of cystocele are those in which the bladder is torn from the pubic anchorages. You have therefore an excellent support. Suppose these broad ligaments are so flabby or relaxed that they pull down, so that you can suture them easily together and easily hang the bladder to them, nevertheless they retract, the weight of the uterus being removed, in an astonishing way. It goes without saying that we can build a very effective pelvic floor by utilizing these pelvic structures.

I would like to voice a protest against gynecologists for forgetting for so many years what Dr. Goffe did and advocated in putting this method before the profession. To Dr. Mayo I made the same protest.

DR. J. WESLEY BOVÉE, WASHINGTON, D. C.—I think we are apt in this work to forget the structures that are the real supporting ones in the pelvis. Dr. Goffe has called attention to that. There is still a possibility for failure even from this procedure when we think only of a part of the pelvic floor, namely, the broad ligaments. We also have supports coming from the sacrum which help to make up the great diaphragm of tissue which must be considered, or we may have the posterior margin of the broad ligament thus constricted pushed down along the vagina. We must hold it well back. Perhaps it ill becomes me to mention the structures running from the cervix back to the sacrum, which are necessary to keep this diaphragm of tissue in good condition. We might easily get the uterus to hang behind the broad ligament shelf unless we see it is attached well posteriorly either by strengthening the uterosacral ligaments by rebuilding them or triplicating them, or in some manner making normal tension on them.

DR. VINEBERG (closing).—I cannot quite understand the nicety of discrimination between interposition and transposition. After all, the uterus is put between the bladder and vaginal wall, and that is an interposition, and I do not see that there is much to be gained by using the term transposition.

As regards the size of the uterus, I would say that any uterus that is above the normal size,—and we should be familiar enough with what is the normal size,—that corresponds in size with the pregnant uterus of five or six weeks, is too large to be interposed. A uterus that has thick walls may not be found enlarged longitudinally, but it has been my experience that enlarged uteri that are interposed do

not atrophy. On the contrary, one of the things which made me turn to this operation was an experience I had of a patient in whom I interposed a uterus that was slightly enlarged, and later on the uterus became so large that I advised operation. The patient was so disgusted with me that she went to some one else and had an hysterectomy done.

The dangers of infection I think can be overcome. In all plastic work in the vagina a certain amount of suppuration will occur. I insert a piece of iodoform gauze at both angles in the anterior vaginal wound between the cervix and vaginal wall and remove it in about 48 hours. That stops the oozing, and also, if there is a collection of infectious material, permits drainage. I believe there is less danger of infection from below than above. If the case is taken in time and handled properly, even if there be an infection, and proper drainage be established, you will not get a fatality from it. I believe in my own case in which a fatality occurred that had proper drainage been instituted the patient would not have died. On the other hand, if you get an infection in a case operated upon from above, a peritonitis develops and your patient is practically beyond hope. The mortality is not any higher than the mortality of fibroids operated from above, which is at least 2 per cent in the best hands.

I have not made any claim of priority, and have given Löwit credit for his work, but I do claim there is an advantage in my method of fixing the stump of the cervix and of amputating the vaginal portion when it is lacerated or hypertrophicd.

Bladder symptoms do occur in a certain number of cases whether you do the ordinary interposition operation, or whether you do this operation, but there have been no serious cases of bladder infection in my series. With a little treatment the bladder irritation will subside.

I am not prepared to enter into the merits of the Goffe operation over that of the Mayo operation. All I can say is that either is an excellent operation, but is indicated only in very elderly women. I heartily agree with Dr. Bovée that when you remove the cervix you remove a great deal of tissue that forms the pelvic support. There is a great deal of connective tissue around the cervix and cervical stump and the base of the broad ligaments which forms a support of great value. I must say that in the cases in which I have done total hysterectomy there was not the same good result, and I tried to follow both the Goffe and the Mayo methods. There is not the same support as there is when you get a good result from the operation such as I have described here this morning. There is a more firm and better support to the pelvic floor than there is when the whole uterus is removed.

Dr. Floyd E. Keene, of Philadelphia, read a paper on **The Interpretation of Vesical Symptoms in Gynecological Diagnosis.** (For original article see page 375.)

DISCUSSION

DR. JOHN O. POLAK, BROOKLYN, NEW YORK.—I think this presentation of Dr. Keene's demonstrates how important it is to include cystoscopy in the department of gynecology and have it intimately related to the work of that department.

There is only one point I want to speak of in the discussion which Dr. Keene passed over casually, and that is the effect of parametrial inflammations on the urinary tract as the result of the contraction that takes place in the sear of a parametritic infection. In other words, a very large number of patients complaining of gynecologic symptoms give a history of the pain being referred to one side, and on urethral examination there is found to be a difference in the function of the two kidneys. The kidney on the side where the cicatrix is, even though the

stricture is not very pronounced, has a tendency to develop frequently intermittent pyelitic symptoms and the drainage is different on the two sides. With the relief of that we have not infrequently seen these symptoms disappear.

There is one question I would like to ask Dr. Keene, namely, does a marked retroflexion change the shape of the urethral orifice?

DR. BROOKE M. ANSPACH, PHILADELPHIA, PENNSYLVANIA.—I have seen some of the work of Dr. Keene and would emphasize the importance of cystoscopy in connection with gynecologic practice. In a large proportion of pelvic cases there are bladder symptoms, and I make a practice of obtaining a catheterized specimen of urine from every patient for urinalysis. This may seem to be a simple matter to the men of this Society, but in outside consultation practice, unless insisted upon, pains are not taken to obtain a catheterized specimen, and voided specimens being contaminated with leucorrheal or menstrual discharge may be more or less valueless.

Sometimes the pelvic condition is drawn to our attention first by vesical or kidney symptoms. I recall one case of fibroid tumor in which the symptoms resembled those of a stone in the bladder, and another fibroid tumor of which the first symptoms were a kidney crisis.

As to whether an acute retroflexion of the uterus alone can produce vesical symptoms, I am at some variance with the opinion expressed by Dr. Keene and believe it often does, and that the symptoms result from the pressure or traction of the displaced cervix upon the base of the bladder. Such symptoms are promptly relieved by restoring the uterus to a normal position.

It is not uncommon to get cases of cystitis and pyelitis associated with pelvic disorders in which the predisposing cause is a stasis of urine from distorsion of or pressure upon the ureters or bladder. A colon bacillus infection takes place secondarily. Very slight pressure on the ureter is sufficient to cause stasis of the urine in the ureter. The secondary infection will usually rapidly disappear as soon as the basic lesion is relieved, and sometimes no more treatment of the ureter is needed than simply the passage of a catheter.

DR. ROBERT L. DICKINSON, BROOKLYN, N. Y.—The cystoscope can be used by the gynecologist as part of his routine examination in every case that has an irritable bladder or bladder symptoms, using preferably the small Broedel instrument. It causes very little or no pain to the woman and one gets a good view of the bladder base with it and can determine whether or not further measures are necessary.

As to the frequency of urethritis of mild type in women with congested bladder base, one gets very good results from using a gloved stretcher dilator, not the ordinary dilator, because the meatus is smaller than the rest of the urethra, but of the type I have mentioned. This with simple applications to the bladder base, gives us in the important cases Dr. Keene described this morning a very large number of cures and the relief is out of all proportion to the amount of work we have to do. There is no use in sending such cases to the cystoscopist if that simple treatment relieves them.

DR. ARTHUR H. CURTIS, CHICAGO, ILLINOIS.—I have been impressed with the fact that after operations one of the most frequent troubles we have is the retention of urine. I would like to ask Dr. Keene how frequently he has encountered this trouble, and what his measures are for relief; whether he catheterizes once or several times, and if he does catheterize, does he continue to do so until residual urine disappears.

DR. EDWARD H. RICHARDSON, BALTIMORE, MARYLAND.—Urology in women is an integral part of gynecology, as well as is general abdominal surgery. These

three branches of surgery belong together and are inseparable, and Dr. Keene's slides and his paper illustrate forcibly how frequently they are associated. Any one who attempts to practice gynecology without the use of the cystoscope as a routine procedure is going to miss perhaps the most interesting group of cases which present themselves with pelvic lesions.

I agree with Dr. Keene in his statement that uncomplicated retrodisplacement of the uterus is not an important factor in the production of vesical symptoms.

One type of case which Dr. Keene mentioned, so far as my experience goes, is unique, and that is the case presenting a large fibroid associated with hematuria in which was found a condition approaching a papillomatous growth over the dome of the tumor in the bladder. I would like to ask Dr. Keene whether he made a microscopic section of that particular portion of the tumor which was protruding into the bladder, and if so, whether or not it turned out to be an adenomyoma. We know of the invasive character of adenomyoma and that clinically at least it is a malignant tumor. The association of hematuria from the mere pressure upon the bladder of the fibroid tumor is certainly an exceptionally rare occurrence. It suggests strongly, therefore, that this tumor may have been an adenomyoma with actual invasion of the bladder wall by the growth, and that the papillomatous appearance on the bladder wall, if examined microscopically, might have shown endometrial elements.

DR. GEORGE GRAY WARD, JR., New York City.—Since Dr. Curtis made his valuable contribution on retention of urine after operation, I have been in the habit of making it a routine in my division in the Woman's Hospital in all the extensive bladder operations, to catheterize at once after the patient voids until the bladder demonstrates that it can completely empty itself. There is no question in my mind from the experience we have had that this is one of the ways of avoiding postoperative trigonitis, which is I think more common with these patients than one realizes. I simply wish to emphasize the great value of this prophylactic measure.

DR. HENRY T. BYFORD, CHICAGO, ILLINOIS.—Ordinary retroversion, without other complications than relaxation of the posterior supports, seldom causes symptoms. When there are symptoms there is usually more or less prolapse with relaxation of the connective tissue anteriorly, or some other pathologic condition in the pelvis.

DR. KEENE, (closing).—Dr. Polak has brought up an important point, namely, the incidence of partial strictures of the lower portion of the ureter in cases of old parametritis. I think that is a point one should investigate in distinguishing left sided symptoms in patients that have been operated upon as well as in cases of chronic infection. In these cases the symptoms, especially those of blockage, are usually associated with the menstrual period. As a rule, the symptoms are premenstrual in type occurring during the height of premenstrual congestion and are due to a partial occlusion of the terminal portion of the ureter. One observer has drawn attention to what he calls renal dysmenorrhea in which the pain is always lateral, due to the fact that there is a partial obstruction of the ureter giving rise to renal colic.

With reference to the remarks of Dr. Anspach as to the question of obstruction associated with an uncomplicated retroflexion, my experience has been that such does not occur.

With regard to the question of Dr. Curtis, we follow his technic in our service. These cases are catheterized as he has recommended and we find it of the greatest value.

Dr. Dickinson brought out the point of the case with which patients can be examined by using a very small cystoscope, for it means nothing more than passing

a catheter, so far as discomfort of the patient is concerned. It takes three minutes, and one determines definitely the nature of the underlying pathology. This fact of dilating the urethra is of enormous value. We meet so often cases at or about the time of the menopause of what we call the climacteric bladder in whom there are marked symptoms; one finds a normal urine, a little edema, with perhaps small cystic formations in the trigone, changes so small as to be out of all proportion to the severity of the symptoms which are presented. In such cases remarkable relief often follows the examination and complete relief may be obtained by repeated urethral dilatations.

In answer to Dr. Richardson's question regarding the nature of the tumor, it was not an adenomyoma extending to the bladder. I quite agree that hematuria in association with a benign tumor of the uterus is most unusual, but in this case I am quite sure that such did occur for the bleeding ceased immediately after operation and did not recur.

DR. EDWARD REYNOLDS AND DR. DONALD MACOMBER, of Boston, read by invitation, a paper entitled Certain Dietary Factors in the Causation of Sterility in Rats—With Special Reference to the Histology. (For original article see page 379.)

DISCUSSION

DR. EDWARD REYNOLDS, Boston.—We perhaps ought to say in self-defense that the numbers of matings which we have made are much larger than we have put on paper.

I would like to speak of one point which there was not sufficient time to bring out in the paper. Abortion with macerated fetus is apparently unknown in rats. We could find no mention of it in the extensive literature of rat laboratories, and Dr. Castle has never seen an instance. We had 8 cases of abortion with macerated fetuses in our small number of matings on deficient diets. We have seen many instances in the human race of defective and scanty spermatozoa in man whose diet was analyzed and found to be defective. Most of them have improved and some of them have returned to normal with correction of the diet. We have seen one instance in a woman of whose history we have an abstract report. She was a well nourished young woman of the well fed class. She kept a chef and maintained an ample table. She had had five successive abortions. We could find no cause for them. It seemed absard to analyze her diet, but an analysis showed that during the years in which she had had these five successive abortions she had selected from the abundance of her table a diet which contained practically no calcium. At this point she became pregnant a sixth time. She was at once put on medicinal doses of carbonate of calcium; her diet was altered to one that contained an abundance of calcium, and in a little while medicinal calcium was stopped. The diet was continued and she went to term and bore a fine child.

A single case proves nothing, but in connection with rat work and work on cattle and swine it is known that a deficiency in calcium causes abortion. This is suggestive and well worth considering.

I would like to say also, we have not had time to introduce war statistics, but we have received statistics of the present birth rate in the central countries, notably in Austria, which show in spite of an increase in the marriage rate since the war the birth rate has fallen off, in Austria from 18 to 12 per cent.

DR. W. BLAIR BELL, LIVERPOOL, ENGLAND (by invitation).—I would like to express my high appreciation of this very valuable and important scientific paper.

Furthermore, I should like to express my approbation of the judicious caution with which the conclusions have been drawn. I think Dr. Reynolds and Dr. Macomber have put the least possible construction on the practical issues arising from their work.

I dare say, many of you know that for fifteen years I have been very much interested in the calcium metabolism. It is exactly fifteen years ago since I published my first paper on the subject, and in that paper I drew attention to the extreme importance of calcium metabolism in regard to the reproductive functions.

First of all, I would call attention to what probably every one knows, namely, that a hen will only lay eggs when it is well supplied with calcareous material. I therefore investigated hens in my laboratory and by devising a method for estimating the calcium in the blood, I was able accurately to say when the hens had laid eggs, and when they had not laid eggs, and I could predict their functional ability. Those experiments of course, led me to examine women, and I found in them changes in the blood which were indicative of the relationship of the calcium metabolism and reproductive functional activity. Menstruation was found to be almost entirely dependent upon the calcium metabolism; that is to say, I found suggestive changes in the calcium content of the blood of women before, during, and after menstruation. Moreover women who suffered with amenorrhea owing to calcium deficiency could be made to menstruate by the administration of calcium lactate in therapeutic doses.

I also found many years ago just as Dr. Reynolds has found, that calcium lactate will prevent habitual abortion.

With regard to the results obtained by Dr. Reynolds and Dr. Macomber with protein deficiency in the diet, it seems to me, that a low protein diet is not of the same importance in this connection as the calcium deficiency. Protein, of course, is very obviously necessary for every function of the human body, and it forms such a large part of the human body that one looks upon a low protein diet as producing a general insufficiency.

I think that these experiments which have been detailed by Dr. Reynolds and Dr. Macomber show that a low calcium content in the diet is the specific factor in causing sterility.

DR. EDWARD P. DAVIS, PHILADELPHIA, PENNSYLVANIA.—I can only revert to published analyses of the blood of pregnant women showing its calcium content, and that these analyses show amply that the various disabilities of reproduction in pregnancy are accompanied by enormous variations in calcium content. My own observation and studies have been along the line of toxemia in which these variations have been exceedingly common, and a study of fertility may throw important light on the vexed problem of toxemia.

DR. MACOMBER, (closing).—I think it might be of interest to enumerate a few of the foods that are rich in calcium. Milk is the chief supply in the human diet, and an ample amount of milk, if taken, will counteract any question of deficiency. Green vegetables also contain moderate amounts, but milk is the chief source.

· As Dr. Bell has brought out, medicinal calcium is either calcium carbonate or calcium lactate, which is a very valuable thing to use clinically, and as Dr. Reynolds has said we have had some success with it.

As to low protein in the food, I think it is demonstrated by our experiments that low protein alone, as Dr. Bell remarked, is not as important as low calcium, but when combined with low calcium it increased the bad effect of the low calcium. That is something that is rather universally appreciated by animal breeders as a whole because they make their diet for breeding animals high in protein.

Dr. Reuben Peterson and Dr. William J. Butler, of Ann Arbor, presented a paper on Pneumoperitoneum and Roentgenography as Aids to More Accurate Obstetric and Gynecologic Diagnosis. (For original article see page 349.) (This paper in the absence of the authors was read by Dr. M. D. Haag of Ann Arbor.)

DISCUSSION

DR. I. C. RUBIN, New York CITY.—I regret Dr. Peterson is not able to be here, because I wanted to learn from him a number of points that have been troubling me since I have done this work. So far, the total number of cases I examined by transuterine insufflation of oxygen and carbon dioxide is 326. In this series there were 192 that proved patent the first time. In 95 cases non-patency was the result. In 25 cases the test was repeated once. In five cases the test was done three times. In one case it was done six times. In 8 cases the result was doubtful the first time and no later examination was carried out to corroborate the finding or to amplify it.

The cases that were chosen were those that were sterile either absolutely or those that had had one child or one pregnancy, that is a miscarriage. In most of these cases of miscarriage there was a history of intentional induction of abortion very shortly after marriage. These form a rather sad group of the sterile cases. In a great number of that group nonpatency was the result. I also used insufflation in a number of cases that had had operations for the relief of sterility or for other gynecologic conditions, but in which sterility was one of the features. For instance, there were a number of patients upon whom a Dudley or Pozzi operation or simple dilatation was performed. In others a stem pessary had been inserted. There were also a number of patients who had had ectopic pregnancy and upon whom a unilaterial salpingectomy was performed. One patient had had a bilateral salpingectomy for repeated ectopic. There was a small group of patients who had had disease of the adnexa and a laparotomy with ablation of both tubes, who were nevertheless still hopeful that they might become pregnant and who displayed an intense desire for future pregnancy.

The usefulness of this method was evident in cases where we had done therapeutic sterilization by ligating the tubes. There were a number of such cases in which it was important to learn whether the ligation was successful, or whether at least one of the tubes or the stumps had become patent. There was a small group of cases in which myomectomy was done as a conservative measure in preference to a hysterectomy because of absolute sterility, the hope still remaining that the patient might become pregnant. Where a number of tumors was removed from the uterus, particularly when one of these was of the submucous variety, it was rather interesting to determine whether in one or two places the sutured bed after enucleation might not have included the uterine ostium or the intramural portion of the tube. Then there was a small group of cases upon whom we had done plastic operations on the tubes, "conservative surgery" cases, which had had bilateral salpingostomy for bilateral hydrosalpinx or where the plastic operation was performed upon one tube in young women in whom we still wished to conserve the function of pregnancy. There were three cases in that group, and in one case only was patency proved. The other tubes proved nonpatent as it was impossible to get the gas through them to establish a subphrenic pneumoperitoneum.

I have not used the method for abdominal inflation except in a few instances. For the purpose of general abdominal diagnosis, I am using 1000 c.c., a little more or less. I do not believe that it is dangerous to do it for this purpose. I believe the peritoneum takes up gas rapidly when one uses carbon dioxide, and Dr.

Peterson has amplified the method admirably and has come to the same conclusion. His patients have not suffered any untoward symptoms immediately, neither were there any sequelae.

My object primarily was to find a method whereby one could determine without operation, if possible, the fact of patency of the tubes, for I have had, as most of us have had, some disappointing experiences in cases that I had operated for sterility by one method or another, and in patients in whom the physical findings before the operation were absolutely negative. Undoubtedly many of these failures were due to nonpatent tubes that had escaped detection.

The points that have interested me particularly in developing the method in the last 150 cases, I am going to refer to briefly, and by the way, I have used carbon dioxide exclusively since it was first suggested. I use very small quantities of the gas, only 100 or 150 e.e., practically a cupful, which rises immediately after insufflation to the diaphragm, lifting it up slightly, as evidenced on fluoroscopic examination. The reaction of this subphrenic pneumoperitoneum varies with individuals. As a rule, the symptoms caused by the insufflation or the subphrenic pneumoperitoneum are very mild, and they result in a little sticking pain in either shoulder or both. These pains are pathognomonie; they are so constant when we use 100 c.c. of carbon dioxide that without resorting to the fluoroscope we may say that the gas went through the tubes as evidenced by these pains. Patients, as we all know, react differently, however, to pain stimuli, and therefore occasionally one can slip up if he depends upon that symptom alone, because some patients are apt to say they have strained themselves on the examining table and have pains in their shoulders. Therefore, I have practically combined the use of the fluoroscope with the insufflation in every case. It is a simple matter.

I have done most of these cases in the office or in the dispensary, or in the x-ray department of the hospital, and most of the cases have been ambulatory.

One point I would like to emphasize in closing is this: if the method is to be of any permanent value it should be along these lines. In the apparatus I have employed, the gas is allowed to displace 40 c.c. of water through the siphon cylinder at the rate of 15 second's time, to raise the mercury column to 100 m.m. This I find is the most desirable rate of pressure flow. When the pressure reaches 80 before the initial drop, I have found so far that the tubes are normally patent. When the pressure of this rate of flow reaches 200 millimeters, the tubes are probably occluded, but at another time with a different pressure rate of flow, say 20 seconds to 100 mm. Hg. or 30 seconds to 100 mm. Hg., we may show the tubes are patent. In other words, the gas may go through at another time under different pressure conditions. When this pressure rises to 140 or 160 before a drop is noticed in the mercury column, there is probably stenosis in either one or both tubes. Occasionally in a nonpatent case, that is, where the pressure is high, we are ready to conclude that the patient has occluded tubes, but on second examination we will find the gas goes through. Therefore, I have adopted the rule not to say a patient is sterile on account of nonpatent tubes unless the findings are uniformly the same by three examinations. The important thing in the further development of the method is the behavior of the carbon dioxide insufflation in cases on which we are going to operate, where the abdomen has to be opened for one or another indication, and to note what happens under the eye by noting the pressure phenomena. After we have had a larger series of cases, we may be able by the present method to predict without doing laparotomy whether we are dealing (1) with normal patency of the tubes, (2) with a relative stenosis in one or both tubes, or (3) whether we are dealing with complete occlusion. The great contribution Dr. Peterson has made in the matter of tubal patency is where there has been a little increase in pressure he introduces enough of the gas so that he can get a picture of the pelvis and of the pelvic organs, and so can determine which tube is at fault.

DR. GEORGE GRAY WARD, JR., New York City.—I was stimulated to try out this very interesting procedure when Dr. Peterson came to New York and presented a paper before the Sloane Alumni Society. I sent one of my house officers to Dr. Rubin to obtain the necessary information as to the technic and the apparatus, which he kindly furnished us. We have tested the method with those cases that seemed suitable for it, and the results we have obtained so far in the Woman's Hospital are as follows:

GAS INSUFFLATION OF TUBES

Twenty-two (22) patients complaining of sterility have been examined. Carbon dioxide gas has been used in eighteen (18) cases and oxygen in four (4) cases. One (1) patient has been reexamined after an interval of one month. Only two (2) cases have been examined at operation following insufflation. No conclusions can be drawn from so few cases but the following observations have been made.

CLASSIFICATION OF CASES

The standards of pressure which Rubin has adopted to judge the condition of the tubes have been used to classify the cases examined. All cases examined in which the level of pressure has not risen to 150 m.m. with a rate of flow of the gas of 20-100 have been classified as patent. Cases in which the level of pressure has risen to or above 150 m.m. but has fallen before reaching 250 m.m. have been classified as partially occluded. A rise in pressure to 250 m.m. has been considered to indicate absolute occlusion of the tubes.

Using this classification the results have been as follows: patent, 14; partially occluded, 4; totally occluded, 4; totally 22.

CONFIRMATORY TESTS

Fluoroscopic Examination.—The determination of patency or occlusion of the tubes as indicated by pressure readings has been checked in all cases by an examination with the fluoroscope. Gas under the diaphragm has been easily observed in all cases believed to be patent or partially occluded, that is, whenever gas appeared to enter the abdomen as judged by pressure readings.

Right Shoulder Pain.—Right shoulder pain has proved to be a very common symptom. It has been present in fifteen (15) of the eighteen (18) cases in which gas has entered the abdominal cavity as soon as the patient has assumed the erect posture. This sign has been so common and so indicative of the presence of gas in the abdominal cavity that as Rubin pointed out fluoroscopic examination has been almost unnecessary when it has been present. This is of practical value from a clinical standpoint when a fluoroscopic examination cannot easily be obtained.

SUBJECTIVE SYMPTOMS

Insufflation has been accompanied by considerable discomfort for a short period of time in a number of the cases. Patients have complained of pain which is caused by the following: 1. Picking up the cervix with bullet forceps. 2. Passage of the cannula through a tight cervical canal. 3. Downward traction on the cervix. 4. Distention of the uterus when the tubes are occluded causing cramplike pains in the lower abdomen. 5. Reflex pain from the diaphragm in the right shoulder.

The right shoulder pain has been most severe, varying from slight discomfort to pain so severe that it was accompanied by nausea in five (5) cases and faintness in three (3) cases as soon as the patient sat up.

No alarming symptoms have occurred in any case.

It has apparently been very difficult for many of the patients to come to the

hospital complaining of sterility when they had no other symptoms. Many of the patients are very apprehensive, and fear examination on account of the pain which it may cause. Many of the unpleasant symptoms can be avoided by going about the examination in a quiet way and reassuring the patient. After examination the patients have invariably soon forgotten all discomfort and have been very grateful to feel that an examination has been made which promises to give some definite information about their condition.

CARBON DIOXIDE VERSUS OXYGEN

Four (4) patients have been insufflated with oxygen. Two had occluded tubes and two were patent. One of the patent cases had no symptoms, the other was neurotic. She complained of pain and discomfort for three days following insufflation.

With carbon dioxide gas considerable discomfort has occurred in several of the cases. However, symptoms have never lasted longer than one-half hour. Carbon dioxide has been used for this reason and also because facilities were such that an immediate examination could be made with the fluoroscope.

VOLUME OF GAS

The volume of gas introduced has never exceeded 480 c.c. The average amount used has been from 240 to 280 c.c. The volume used has proved to have very little to do with the symptoms produced. Carbon dioxide is absorbed so rapidly that symptoms have readily disappeared, although a relatively large amount of gas has been used. This is important when an obese patient is to be insufflated since examination with the fluoroscope is difficult.

VALUE OF METHOD FOR PRODUCTION OF A PNEUMOPERITONEUM

Unless it is important at the time of examination to know also whether the tubes are patent, this method has not seemed practical for the production of a pneumoperitoneum. With the use of local anesthesia, gas has been introduced by needle through the abdominal wall with comparatively much less discomfort to the patient.

OBSERVATIONS IN INDIVIDUAL CASES

Case 1.—The uterus was very small and sensitive to pain. Cramplike pains were so severe that the pressure could not be raised to 250 m.m. with either trial.

Case 3.—This case was given a second trial after an interval of one month. The findings were the same at the second examination.

Cases 5, 7 and 18 give histories like that of ovarian hypofunction. The low level of pressure is interesting.

Case 8. Here a high level of pressure was needed for penetration. The possibility of breaking down adhesions at the end of the tube would be an interesting interpretation of the findings.

CASE 10 is the record of a case which showed congenital occlusion of the tubes at operation.

Case 11 is a case in which chronic bilateral salpingitis had been diagnosed in the clinic and in the house. Examination showed the tubes to be patent. Inspection at operation showed the tubes normal.

Case 12 shows how a false interpretation could arise if the cannula was in a long tight cervix instead of the uterus during insufflation.

Case 14 shows tubes patent. A preoperative diagnosis of left adnexal disease had been made. At operation the adnexae were found normal.

Cases 15 and 16 showed two occluded cases in each of which the bimanual examination was negative.

Case 17 was a case in which a "right salpingectomy and a resection of the left tube and ovary" had been done three years previously. Gas passed into the abdomen.

Case 21 showed a rather low level of pressure. A diagnosis of retroversion and adnexitis had been made in the clinic. Much involvement of the tubes seems doubtful.

In closing, I wish to say that I consider Dr. Rubin has made a very valuable contribution to our methods of diagnosis, and I cannot see how we can have any woman coming to us for sterility, where we have checked up the husband, without previously trying out this method to prove the tubes are patent before we subject her to various operative procedures either on the cervix or via the abdomen.

DR. EDWARD REYNOLDS, Boston, Massachusetts.—When gentlemen have done hard work and a great amount of it in developing a new method, it is always ungracious to present even in the most moderate way opposite views, and it is ungracious when one's personal experience is very limited as compared with the work the gentlemen have shown here.

My experience is limited to a demonstration of three cases by Dr. Rubin, and I speak as a spectator of this method. I believe the method is demonstrated to be reasonably safe. I believe it is feasible, and that the evidence produced is probably in most cases valuable and reasonably dependable. However, I do not think it is a simple method. It demands special training and good training of assistants, and a good deal of training for the operator, so to speak. It involves a large amount of apparatus and care. Even after the training I cannot believe that it is an easy or simple matter for the patient. My impression was the patients suffered a great deal of pain, and that they were considerably prostrated afterward temporarily. I believe it to be a method of value and worthy of further exploitation. I feel it is not a method to which I should personally wish to subject a woman in whom I was interested unless I thought it would furnish evidence of distinct value to her interest. I think there is a very limited class of cases in which this evidence is necessary.

I protest against its use from my present knowledge as a routine procedure in cases of sterility. I think if a man has trained himself to accurate examination of the secretions of the genital tract in a woman he can obtain in most cases specimens which he has a fair right to believe are obtained from the body of the uterus and very near the fundus, specimens that are not contaminated, and also from the confirmatory evidence of the microscope that show there is no essential contamination. If you have spermatozoa at the fundus of the uterus in postcoital examination, and examine the secretion microscopically, you may feel sure that the tubes are not closed. I speak from a large experience in such examinations and no small experience in checking them by abdominal incision. A closed tube is always a tube with an amount of secretion in it. That secretion drains into the uterus and I believe it kills the spermatozoa almost always. If it has been diluted by much more abundant secretions of the uterus and cervix, you will find the spermatozoa within the cervix. If they penetrate to the fundus they are dead. It is usual, if not always, that they come in contact with concentrated tubal secretion which has killed them and you probably have closed tubes. If you have spermatozoa living at the fundus you may be sure the tubes are not closed.

I have recently seen a case in which the tubes were occluded near the cornu, with spermatozoa living at the fundus and tubes closed. With both methods we are liable to make mistakes. Both have their limitations of usefulness and I would not make a routine examination like that proposed.

DR. ROBERT L. DICKINSON, BROOKLYN, NEW YORK.— I would like to report 77 tests of patency of the tubes, 3 of them in Dr. Rubin's office.

In answer to the last speaker (Dr. Reynolds) the method has proved simple without a number of assistants. The first patient was a highly neurotic, badly frightened woman, pallid, and almost scared to death as she lay on the table. She had purple lips and some shock. In half an hour or three-quarters of an hour she was all right. She was a woman who suffered pain and was shocked. She was as bad a subject as you could find for intrauterine manipulation.

The other two suffered no more discomfort than you would find from introducing a uterine sound into a nonsensitive uterus. There was no pallor, no disturbance. In all three cases it was perfectly simple and easy to see the beautiful bow under the diaphragm through the fluoroscope under the gas as it moved. Objection has been raised that the Rubin test involved considerable apparatus and a number of assistants. That is not true. In using fluid for injection, not more than 10 minims enter, and if there is resistance you begin with eight minims, and in four or five minutes more the tubes are open. The maximum amount I have injected is 10 minims. The reaction is more than with gas. One-third of the patients have discomfort two or three hours later. I have had one marked reaction in a patient who had to go to bed, and one other who had considerable discomfort.

I do not believe a test like this, as Dr. Reynolds has said, is for the nonexpert. These tests must be worked out and tried and the cases must be very carefully selected. We have proved that this test can be carried out, and above all things we think there can be no longer any excuse for operating for sterility without resorting to the test to determine tubal patency.

DR. EDWARD H. RICHARDSON, BALTIMORE, MARYLAND.—I wish to say a few words in regard to this method. Through the courtesy of Dr. Rubin I saw this method demonstrated at the Mt. Sinai Hospital Dispensary, and my impressions were decidedly different from those of Dr. Reynolds. I talked with the patients after insufflation was completed, and I questioned them personally to find out whether they had much discomfort. They were standing up and would walk down to the fluoroscopic room and leave the hospital apparently with no more discomfort than a slight sensation in the shoulder. None of them showed any reaction, and I must say I think this is one of the most important contributions that has been made to the study of sterility within recent years.

I was much interested when Dr. Rubin's first paper came out, but I was skeptical about the safety of it. But we have had reported today from the Ann Arbor Clinic and from Dr. Rubin's clinic over 500 cases in which this method has been tried, and the testimony of these gentlemen is that there has been no serious result. I immediately obtained the apparatus and had it taken to Baltimore, and I tested it out on my first case just before I came to this meeting. It did not require any particular skill; it did not require a good deal of assistance, but it is simply a matter of carrying out the technic, and my experience in this one case leads me to believe that this method will come into general use in properly selected cases, and I think its safety has already been demonstrated.

DR. JOSEPH L. BAER, CHICAGO, ILLINOIS.—May I offer a word or two in defense of the simplicity of the method? I had the opportunity of talking with Dr. Rubin in Chicago last summer when the American Gynecological Society met there, and following that meeting I began using the procedure without having the benefit of seeing Dr. Rubin or others more expert than myself do the work. I am doing it entirely in dispensary surroundings and have—only a nursing assistant. I have found it entirely feasible to carry out the test without any further aid.

The pain following the use of oxygen was easily controlled by a single dose of 5 grains of aspirin, and in no case has any shock resulted.

I should like to know if there is a distinct advantage in the use of the Sims position as against the dorsal position for the transuterine gas introduction. I have been using the dorsal position.

DR. HAAG, ANN ARBOR, MICHIGAN (closing).—With reference to what Dr. Rubin said about determining the patency of the tubes, we have made it a routine in cases where the rise of pressure has been very great, say up around 200 m.m. of mercury, to make at least four or five trials. We have found in our initial attempt to introduce gas into the tubes that it did not result in the gas passing through into the peritoneal cavity, but on the fourth attempt it passed through it at a relatively low pressure. In most cases we have used the transuterine or transperitoneal method and checked up the findings by operation.

I want to explain that some of the cases are labeled pyosalpinx, cystic ovary, and so forth, because they were diagnosed that way in the first diagnosis but checked up at operation and gone over carefully and labeled afterwards.

It is also interesting to know that the Department of Medicine and the Department of General Surgery of the University of Michigan were very much antagonistic to the method at first. Now, they are bringing their obscure cases over and asking us to inflate the patients, and we have been able to make some diagnoses for them. We have one case of mesenteric cyst in which an absolute diagnosis was made by the transperitoneal method before operation.

One interesting thing we have noticed is the fact that obstruction a great many times occurs in the interstitial portion of the tube and not at the fimbriated extremity.

As regards the use of oxygen and carbon dioxide, if it is necessary to make an injection in the office and send the patient to a roentgenologist, oxygen is preferable because if there is any lapse of time between the injection and the fluoroscopic examination, you may miss your picture if you use carbon dioxide.

We are now making our own exposures, taking all exposures in stereo, so that we can become deft in examining the x-ray plates. One can get along very nicely without much assistance after he has once learned the method.

We have been using the method more or less routinely, not for the purpose of drawing definite conclusions but to get a large series of cases so that the members of the profession can tell whether they wish to adopt the procedure or not.

DR. RICHARDSON.—Is not this test carried out for the deliberate purpose of determining the patency of the tubes?

DR. HAAG.—We have only one case in which the abdomen was opened and we had the tubes directly in sight in which the fimbriated extremities were sealed. There was a small opening through one tube. I cannot tell the exact number of millimeters, but it is some where around 220. We would not get above 200 millimeters unless we could watch the tube. In that case the tube was forced open under 220 millimeters of mercury, but was under our direct sight where we could see what was going on. The fimbriated end was folded in and was sealed off.

As regards the question of Dr. Baer's, concerning the efficacy of the Sims or dorsal position, that is a personal matter. We tried the dorsal position first and tried the Sims position, and we found we could expose the cervix much more easily in the Sims position than in the dorsal position. That makes no particular difference as long as you get it done, but we like the Sims position better than the dorsal position.

We have not attempted to draw any conclusions. However, I do think from what we have seen in the clinic, there is one part of this procedure that will be used quite extensively, and that is the injection of gas through the uterus to determine the patency of the tubes.

THE NEW YORK OBSTETRICAL SOCIETY MEETING OF APRIL 12, 1921

THE PRESIDENT, DR. FRANK R. OASTLER IN THE CHAIR

Dr. William H. Cary, of Brooklyn, N. Y., read a paper entitled **Sterility Studies—Simplified Methods of Diagnosis.** (For original article, see p. 406.)

DISCUSSION

DR. I. C. RUBIN.—In the matter of the insemination test: Dr. Cary is, of course, familiar with the fact that Dr. Hühner of New York City, practically originated that test in 1912. There is very little difference, it seems to me, between his method and Dr. Hühner's which I have been following since 1912.

I agree with practically all the suggestions and the rules laid down by Dr. Cary in following up a case of sterility. I am not prepared to agree with him about the percentage of cases in which he said infertility might be ascribed to the husband or to the wife, nor in the percentage of cases that the sterility may be said to be due to vaginal pathology, cervical pathology, tubal pathology and ovarian pathology, including endocrinology of a pathologic character. I have studied several hundreds of cases, but am not prepared to draw definite conclusions on those points.

The matter of testing tubal patency interested me tremendously. This method is an exceedingly simple one, and from my experience with both the injection of fluid into the uterus and of gas media, I can corroborate his findings.

I am not so certain, however, that 16 minims is the arbitrary limit of the amount to be introduced, for the reason that if you take a number of extirpated uteri and test their capacity—unless Dr. Cary means a nulliparous uterus which is virginal in character, the capacity of which, without being distended is from 10 to 15 minims—you can distend many a uterus before going in through the tubes. That would be more than 16 minims. I have tried that out a number of times, and before you go in through tubes which are practically normal, you can distend the uterine cavity sometimes with as much as 10 or 15 e.e., and that is a lot more than 16 minims. For example, I had one case, in which the tubes were obliterated and in which the uterus was not too large, but atonic, and in that case I put in 40 e.e. So I think that 16 to 18 minims would lead to error in the hands of less expert observers than Dr. Cary himself.

One of the reasons why I abandoned the injection of fluid was because of the colic and the local irritation and pain it caused when I used argyrol. In February, 1914, I did the collargol injection on rabbits. In April the work was published in the Zentralblatt für Gynäkologie. Dr. Cary I believe published his paper in March. It was a very striking coincidence that our work was practically done about the same time. My work was done upon animals, and I started from an entirely different angle at first. I was first interested in excluding submucous myomata from cases of fibroids that were to be subjected to roentgen therapy because I had learned in studying radiotherapy for fibroids that submucous myomata did not yield to x-ray therapy and that they often sloughed and finally had to be removed by operation.

Collargol causes peritoneal reaction, but I agree with Dr. Cary that the peritoneum takes care of it. There were two objections to collargol from my viewpoint. I abandoned collargol later for argyrol, then bromid solution and thorium nitrate solution. One was that it left inspissated deposits in the tubes, which I thought might lead to occlusion where none existed before. That Dr. Cary has eliminated by using plain saline. I do believe in some cases where there are normal tubes and a mild

degree of peritoneal reaction, that you may get a sealing off of the fimbria where that did not exist before. I do not believe, however, that there is in that any danger to the patient. I recall one case where a housestaff man curetted a patient preliminary to an interposition operation and kept a rather wide calibred irrigating tube in the uterus without allowing for the return escape of the fluid. I was able to demonstrate to his satisfaction in a few minutes, as I was quite sure was the case, that there was a great deal of the fluid in the peritoneal cavity, by exposing the uterus through an anterior vaginal incision. There is no doubt that Dr. Cary's method can be used as a test and is without the disadvantages of collargol and other opaque substances.

On the other hand, I think the capacity of individual uteri varies, so that 16 minims would not be sufficient to go through, in the first place, in a number of instances, and, in the second place, you may get (I do not believe it will be as common) a sealing off at the wrong point without any morbidity that is noticeable.

The advantage, from my viewpoint, of the gas, $\mathrm{CO_2}$ or $\mathrm{O_2}$ over fluid media is that it rises immediately to the subphrenic space. If there is any danger it might arise under the diaphragm, but at least the patient's tubes will not be sealed off. The carrying of infective material by the gas to the subphrenic space is a matter for speculation. In nearly 300 cases I have not seen any ill results. I think in that one respect there is a satisfactory advantage of the gas media over the fluid media.

DR. ROBERT L. DICKINSON.—There can be no question that the Rubin method, using the fluoroscope, is a mathematically exact demonstration.

Dr. Cary's emphasis, I take it, is on simplicity. His hydraulic test seems to me as sure as Rubin's gas, but it takes more sensitive fingers and more experience.

I had the pleasure of seeing Dr. Rubin do his quick, expert work in his own office. Of the three patients tested, two had no more reaction than from a vaginal douche. One woman was neurotic and very apprehensive and jumpy beforehand. She went clammy and blue and it was half an hour before she straightened out. She had right shoulder pain.

I am sorry Dr. Rubin did not quote Dr. Reuben Peterson's endorsement and corroborative experience.

In attempting artificial impregnation I have lost the fear of peritoneal reaction—timid at first, very gentle on pressure, afraid of carrying more than a few drops into the tube or peritoneum. Having tried with semen and with water some 65 times to carry fluid in, and having succeeded in those in whom the tubes were open, after I dared to use sufficient pressure, I have had but one woman develop anything that would suggest salpingitis or localized peritonitis, that is, only one having to lay up at all. Fifteen minims into the tubes is the maximum injected, and four to five should be the preferred amount.

In doing the Cary test a little distress is common, but no more than silver nitrate produces in a cervix. The amount of pain we used to have in the old times in injecting iodine with a pipette into the uterine cavity or in swabbing the cavity of the body with iodine and carbolic was much more than you ordinarily have in these cases. It will not do to advocate the use of either method—either tubal insemination or tubal patency tests except by skilled hands. Dr. Hirst told me of an artificial impregnation by a woman doctor that developed a blazing pelvic peritonitis and after some weeks he had to take out the tubes, ovaries, and uterus. I understand there was a similar result in a case at Mount Sinai. There is no question but that we have to be sure we are not dealing with active pus in the tubes that may be pushed out into the peritoneum. We have to be sure that we have a healthy cervix. We have also to take care that we touch nothing on the way until we get inside of the cervical canal. We go wearily with a method like this until we know what its risks are.

In the matter of sudden dilatation of the whole uterine cavity vitiating the Cary

test, I disagree with Dr. Rubin. I think I know that a uterus does not dilate under pressure. It does in curetting for abortion. We see this when the curette wanders about in the uterus in a flabby-walled cavern. That kind of thing doesn't happen with the nongravid uterus. With closed tubes you get in 10 minims and no amount of pressure will push in more. This proves that dilatation does not occur. With tubes open, a few, 6 or 8, or rarely 10, minims have passed out of your pipette; there is resistance. Then it is gone. When four or five minims more pass, you know they have passed somewhere beyond the uterine cavity. How do you know? Because you try to recover that material from the uterine cavity and it is not there. I can get back 5 and sometimes 8 minims. The rest is not there. There is not a drop of it in the vagina. A perfect illumination with the head light proves it, and also that Dr. Cary's strip is unnecessary.

Bovée and Stone many years ago used tubal injections of iodine when they were about to open the abdomen. They could open some closed tubes. They found that iodine did not produce adhesive pelvie peritonitis, and if iodine does not do it, sterile fluid is not likely to.

DR. H. D. FURNISS.—I went to see Dr. Rubin do this work and was impressed with the simplicity and the inochousness of it. We are doing this work in our clinic and office and it is a rule that no operation for sterility is to be done until the patient has passed this test. Whether you believe in some form of pessary, curettage or cervical plastic, this test will obviate many unnecessary operations.

A few days ago, I tested out a woman with secondary sterility with the Rubin test and it was positive at 100 millimeters. When the test was over and the cannula withdrawn from the uterus, I heard so much bubbling from the uterus of a large amount of gas that I think there was probably more than 16 c.c. in the uterine cavity.

I saw Dr. Rubin's apparatus. He had been reducing the pressure of the gas tanks from 800 pounds into another tank down to about 100 pounds. Many manufacturers make a pressure reducing apparatus to be attached directly to gas tanks. With this, pressure can be reduced from 800 pounds to zero and nicely regulated for any point between. By using in connection with this a Tycos or mercury manometer, the apparatus can be regulated to within one millimeter or less. In performing the test the maximum pressure is set at 200 mm.; no matter what rate the flow.

We use the same pulsating meter as Dr. Rubin. If we are sure that there is no leak in our apparatus, and that no gas escapes at the external os, I do not think it necessary to prove the passage of gas by fluoroscopy. If the meter shows its passage, it must have gone into the abdominal cavity. Nor do I think it necessary to use the amount recommended by Dr. Rubin—40 c.c. being enough.

To prove there is no leak, put a bivalve speculum in the vagina, elevate the pelvis, and flood the vagina with a mild antiseptic solution, above the external os. If there are no bubbles there is no leak.

I think we can simplify the apparatus in this way. Use a 50 c.c. syringe, known not to leak. Fill this with $\mathrm{CO_2}$ by having the gas displace the piston—care must be taken that the flow from the tank is slow and the pressure low. Connect the syringe to the cannula and inject. Through a T connection, a manometer can be used to show the pressure. Leakage at the external os can be demonstrated by the method of flooding the vagina above mentioned.

DR. H. N. VINEBERG.—Dr. Dickinson referred to the case of artificial impregnation in Mount Sinai Hospital with development of a serious infection. I want to speak of this case. It was one of Dr. Hühner's cases. Since that case he has given up artificial impregnation. It was one of the worst cases I have ever operated on. She had an inflammatory mass reaching to the umbilicus. In that instance there was a tuberculous obstruction of the tube followed by this tremendous infection. We kept

her under observation for some time and finally had to do a most radical operation to save life.

DR. JOSEPH BRETTAUER.—I was very much impressed by Dr. Cary's paper, and even if we do not succeed in relieving these women of sterility, we will have gained much if this Society puts itself on record emphatically, regarding the positive harm the curette does in these cases when it is employed indiscriminately.

We have all seen patients that make the rounds and have been curetted twice, even three or four times, until they either become amenorrheic, temporarily or permanently, or else become infected.

I believe that closed tubes in the absence of any history or evidence of gonorrheal infection, must not necessarily be the result of a colon bacillus infection. I believe that physiologically there is a possibility of closure of the tube as a result of the ovulation process; the importance of this has been underrated and underestimated.

DR. WILLIAM H. CARY.—If there is one thing especially that I do not want to appear to do or be accused of doing, it is to not give another investigator credit for what he does. I made a special effort in the conclusion of this paper to state that this insemination test was not original except in its application, and in the publication of this paper Dr. Hühner's work will be given full credit. I only claim originality for its application in the sense of its being a standardized technic, carried out and established, I think, on something like a practical working basis.

In regard to the capacity of the uterus, I can only say that I have injected these patients before opening the abdomen and have measured the amount injected, and I am sure that the men who have assisted me in my operations will bear me out that when as many as 16 minims have been injected fluid escaped through the tube, and if you see it thus demonstrated it is most convincing. Sterility cases are usually nulliparous women. The pipette takes up a great part of the capacity of the uterus to begin with. Now, the French have done extensive work in measuring the capacity of the uterus and they state it as an average of 8 minims. I have doubled that amount in the test, that I suggest, in an effort to be on the safe side; and aside from the question of the amount injected, it is the simplification that impresses one, when with the proper technic and the most gentle pressure you see the saline disappear in the rather dramatic way in which Dr. Dickinson expressed it.

I am one of the first to subscribe to the ingenuity of Dr. Rubin's method. I simply feel this is another means, perhaps a simpler one, which I think may be utilized for proving tubal patency.

NEW YORK ACADEMY OF MEDICINE SECTION ON OBSTETRICS AND GYNECOLOGY STATED MEETING, HELD APRIL 26, 1921

DR. HAROLD BAILEY IN THE CHAIR

DR. JOHN H. TELFAIR reported a Fatal Case of Rupture of the Uterus.

This patient, thirty-two years of age, para v, went into labor at 6 A.M., August 15, 1919. At 2 P.M. the patient was delivered by podalic version and extraction. The physician who delivered her left shortly after. About an hour after delivery the husband of the patient notified the attending physician that there was something wrong and asked him to return. The physician said it was not necessary and did not respond. About two hours after delivery the husband went to the physician and told him he thought his wife was dying. The physician

said he would come after dinner. At 7 P.M. the woman was seen by the attending physician, who immediately called an ambulance and sent her to Fordham Hospital. When admitted to the hospital at 8:00 P.M. she was in profound shock. I would venture the opinion that had this woman been seen within an hour or so of delivery her life might have been saved.

Examination upon admission to the hospital showed a deep laceration of the left side of the cervix extending upward into the lower uterine segment. Immediate laparotomy showed the laceration extending into the anterior uterine wall. A rapid hysterectomy was done and infusion given, but the patient died one hour later.

Opinion as to the treatment of rupture of the uterus is divided. There are those who believe in laparotomy for all cases, and those who believe in packing, if the peritoneal cavity is not entered. If the rupture is not complete, and can be thoroughly explored by the fingers, it may be safe to pack it, but I do not feel that any case that I have seen could have been safely handled in that way. The gross mortality from rupture of the uterus is about 55 per cent. Owing to the difficulty of determining the extent of the injury, I reel that it would be best to subject every case to laparotomy.

DISCUSSION

DR, H. C. BAILEY .- Dr. Williamson reported two similar cases occurring on our service, that were packed. In both instances the rent extended beyond the vaginal vault, being incomplete tears, and one recovered. In one there was a great deal of bleeding at the time of rupture. She was thoroughly packed as there was an extensive tear into the broad ligament. She recovered after a long period of fever. Then she had a thrombophlebitis of both legs, and she finally died after a long-continued stay in the hospital. We have had a number of cases at Bellevue, where, as on the Fordham service, they are likely to come late and our experience has been unfavorable. However, I recall one case that recovered where the uterus was ruptured following the delivery of a large baby. The woman was brought in with the baby's head removed and the shoulders locked in the pelvis, and she recovered following hysterectomy. I had a case recently where, following forceps attempts, a version was performed. It was immediately recognized that there was a rupture of the uterus. I saw the woman about three-quarters of an hour later. On opening the abdomen there was a large interligmentous tumor of blood extending nearly to the umbilicus. The uterus was removed without difficulty or further bleeding. The posterior part of the uterus between the two uterine arteries remained untorn. This woman died of shock.

It seems to me a certain proportion of these cases with suitable treatment by packing could be saved in this way without the added shock of hysterectomy. If treated by packing, of course, the hemorrhage must be controlled, and there are instances in which this has been done by means of clamps. I know of one instance where clamps were used and the patient recovered.

DR. TELFAIR also reported a case of Complete Blindness Occurring During Pregnancy.

The patient was a woman, thirty-six years of age, para v. Her family history showed cancer and tuberculosis. The patient had had searlet fever in childhood. She had four living children and had had three spontaneous abortions. Soon after becoming pregnant she noticed blurring of vision in the left eye. Following this the right eye behaved in a similar manner, so that when she was ten weeks gravid she was completely blind.

General physical examination showed a marked pyorrhea alveolaris and signs in the chest suspicious of a tuberculous process in the right lower lobe. Examination of the eyes showed complete optic atrophy. The urine output was normal and repeated examinations were negative. The blood Wassermann was negative and blood pressure normal. The spinal fluid was under slightly increased pressure but was otherwise negative. The x-ray examination showed no pathologic lesion in the skull, there being no evidence of intracranial pressure and the sella turcica normal. The blood chemistry examination showed normal results. The neurologic examination suggested the possibility of a tuberculous lesion involving the optic chiasm. Because of the marked pyorrhea, most of the teeth were removed and the others treated, but this had absolutely no effect upon the vision of the patient.

She was admitted to the hospital on February 17, 1921, and is due April 30.

DISCUSSION

· DR. CHARLES GEAEF .- The question is whether the blindness is due to the pregnancy or to some other cause. Optic atrophy is roughly divided into two classes. Primary or simple atrophy, which comes without any inflammation, usually due to such conditions as locomotor ataxia, paresis, or multiple sclerosis, etc.; and secondary optic atrophy which follows inflammations of the nerve due to many causes. The disturbance of the uterus in pregnancy, and the toxemia of pregnancy are often among these causes. We distinguish these two types of optic atrophy by certain appearances of the fundus. The fundus in the case of this woman has the appearance of a secondary atrophy. In primary optic atrophy there is a thinning of the optic nerve as it comes through the lamina cribrosa giving a so-called pepper-box appearance to the optic nerve head, with shallow cupping. In the secondary form which follows inflammation, there is an exudate and a deposit of connective tissue formation on the nerve head. In both forms the color is much the same, but there are these differences in the appearance of the nerve head that show if it is due to preceding inflammation. In the secondary form the vessels are small, corresponding to what one would expect in a secondary process following swelling and pressure, while in the primary process the vessels are not much affected. One cannot say with absolute certainty, of course, but the indications are that this is a secondary optic atrophy. She must have had the optic neuritis at the time she became pregnant or soon after. These cases usually occur in the later stages of pregnancy. If it were a primary optic atrophy, there would be no hope of recovery of vision. Since it is secondary, there may be a few nerve fibers remaining alive, and as you well know after delivery there is a general stimulation of absorptive activity throughout the body; it is possible that this may have some effect upon the optic nerves and that she may thus recover a certain measure of sight. It is not very promising however.

As for the causation I can only add that the absence of albuminuria or other toxic blood condition as found by Dr. Telfair when she came under his care does not prove that this patient was free of these throughout her pregnancy. She may have had such a condition when the optic nerves first suffered; this disappearing in the later months.

Again, changes in the generative apparatus are often the source of more or less serious eye affections even when tests of blood, urine, etc., are negative, and if this obtains independently of pregnancy we may accept it as occurring with that condition at times also.

DR. H. C. BAILEY.-I had a very peculiar case that illustrates what may

^{*}Patient was successfully delivered, but continued totally blind when last seen on September 12.

occur from the functional standpoint. A woman who had a preeclampic condition in her first pregnancy, and also in a third pregnancy, had in her second pregnancy at the eighth month, one hour after rising, complete blindness. She was taken to the hospital, and when she arrived she had to be led in. In an hour or so she began to see a little. By 4 p.m. the blindness had quite cleared up. Her eyes were examined in the evening and nothing could be found in the eye grounds. The next day she could see, though she could not read. On the fourth day she returned home and went through an uneventful labor. We explained it on a functional basis. The woman's husband was in the war and she was going through this pregnancy feeling that she did not desire to see the light of another day, and so she had this functional disturbance and did not see the light. She had a normal blood pressure and no albumin in the urine.

DR. GRAEF.—I have had some similar experiences. If one does not find anything in the fundus and sudden, passing, blindness occurs, it is due to uremic or circulatory disturbance in the optical cortex in the occipital lobes of the brain, or else it is a case of hysteria. Hysteria of that type is very common. People will get suddenly blind and then by and by the blindness will clear up without visible change of any kind in the eyes. This patient absolutely had no uremia and so we put it down to hysteria or fleeting circulatory effects. There is an interesting type of visual disturbance which we call "scintillating scotoma." The patient will often tell you that she has headache preceded by seeing zig-zag scintillating spots. This condition is due to a circulatory disturbance in the occipital lobe. In Dr. Bailey's case the fleeting character of the disturbance indicates that it is circulatory in origin. Of course, you know that there is much circulatory disturbance during pregnancy, and the blindness may be due to such a disturbance or else it is hysterical in a case of the type described.

Dr. O. M. Magid presented a report of Ectropion of Abdominal Viscera.

I am presenting this specimen because of its rare occurrence as evidenced by the small number of cases reported in the literature and also because of the diagnostic difficulties it presented in the first stage of labor.

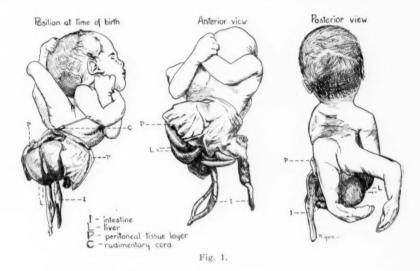
The mother of this stillbirth, M. B., para i, twenty-seven years old; married in 1917. In November, 1919, she was operated upon for an ovarian tumor on the right side, and chronic appendicitis. Her menstruation continued regularly until May 18, 1920. During the pregnancy she felt perfectly well. On the twentieth of January, 1921, I had occasion to see her owing to severe pain from extrusion of internal hemorrhoids. While making a general examination, I found that the size of the abdomen did not correspond to the period of gestation as stated by the patient. I could not hear any fetal heart sounds although the patient said she felt life.

On Feb. 1, 1921, the membranes ruptured. On auscultation the fetal heart could not be heard. Again the apparently small size of the abdomen impressed me. I sent the patient to the hospital. By vaginal examination I observed a thinned out condition of the lower uterine segment; the cervix with its closed os, was firm and button-like; the presenting part was soft and yielding, suggesting a breech presentation. The patient had irregular labor pains until the morning of February 5, when they became more regular and severe. The examination revealed a dilated os of about three and a half fingers, and extruding from it was a mass about the size of an orange. Adjoining this was a smaller mass about the size of a peach; there was a sulcus between them. The two masses were somewhat firm and seemed to be joined together higher up. The patient bled considerably at the time; al-

though her pulse, 90 per minute, was regular and of good quality and she did not appear to be suffering from loss of blood.

It is needless to relate the numerous possibilities of diagnosis that came to my mind while pursuing a watchful waiting policy. In the course of one hour, the two masses presented themselves at the vaginal opening and with them, came a loop of what seemed at first to be the cord, but was soon recognized to be small intestine, the two masses being lobes of the liver. Soon after the above structures were born, the remainder of the fetus was expelled in the transverse position in extreme extension.

Instead of the cord, there were a few strands of tissue connecting the body of the fetus to the mother. These I tied and cut. The placenta was large and showed extreme fatty degeneration; it was expelled spontaneously forty-five min-



utes later. The patient made an uneventful recovery and left the hospital on the tenth day.

The fetus presents a normal appearing head, chest and upper extremities. One would judge it to be that of an eight months' gestation. Instead of the abdominal wall covering the liver and intestines, there is just a thickened peritoneal tissue layer. This apparently ruptured in utero. There is an absence of the bladder and genitals; the right lower extremity has no hip joint articulation and the buttock is missing. Both extremities are rotated so that the feet point toward each other.

DISCUSSION

DR. BAILEY.—The specimen shows a number of things of considerable interest. First the vessels go directly from the placenta to the fetus without any cord. This is a very rare formation. The fact that the entire abdominal contents are in the ectropion indicated not only a failure of the visceral arches to unite, but is further due to a defect in the allantoic stalk and there is a retroflexion for that reason, for as you notice the fetus is bent almost double with the back greatly curved. It seems that the amnion joins directly to the abdominal wall along the edge and one might reason from that that it was due to adhesions of the amnion and that the placenta had failed to become separated. In other words the fetus was attached directly to the placenta without a cord.

Dr. Alfred C. Beck read a Preliminary Report on the Treatment of Syphilis Complicating Pregnancy. (For original article see page 416.)

DISCUSSION

DR. JAMES B. GIVIN.—1 have followed up some of these cases, and have been struck by the absence of luctic stigmata as compared with another set of children of syphilitic mothers who came to the pediatric clinic and who showed four-plus Wassermann reactions.

An interesting feature in this series of cases is the ease with which these babies responded to antisyphilitic treatment. They were given inunctions of mercury and not salvarsan. These observations have shown the importance of continuing antisyphilitic treatment of the mother after the baby is born. The babies cannot be kept on the breast when the mothers are resistant to treatment. We hope to be able to follow this series of children for a number of years and it may be that later these children will show evidences of retarded syphilis. If any of the late signs of syphilis appear it will emphasize the importance of following up these cases, keeping them under treatment, and repeating the Wassermann tests at stated intervals.

Dr. Beek's statistics are interesting in comparison with those published by Dr. Jeans, of St. Louis, who has done a great deal of work in congenital syphilis. Dr. Jeans shows from his personal work and a review of the literature that 30 per cent of all pregnancies in syphilitic families terminate in death at or before term. Dr. Beck's cases show quite different statistics. Dr. Kolmer in the American Journal of Diseases of Children, May, 1920, shows that about 5 per cent of all children coming into a clinic in a large city are syphilitic and that about 5 per cent of the cases coming to a prenatal clinic are syphilitic. The children of these mothers, if properly treated, certainly will not show evidences of lues.

DR. GEORGE W. KOSMAK.—I am interested in Dr. Beck's paper, particularly in the statistics be presents, and more so in his conclusion. I was afraid he was going to say that he had 32 cases of undoubted syphilis but I notice that he modified that assertion somewhat. It is scarcely fair always to assume a positive diagnosis of syphilis simply because there is a four-plus Wassermann reaction.

At the Lying-In Hospital we find the usual proportion of positive Wassermanns and a large number of Wassermann tests are made, but all cases are not tested owing to the fact that many are emergency cases. The babies, however, are tested not later than two to four days after delivery. Many positive Wassermann babies are strong and vigorous and show no evidence of lues. Therefore it makes us rather doubtful whether we ought to make a diagnosis of syphilis where there is a positive Wassermann but no other evidence of syphilis. In women it is very difficult to get a history of a primary lesion and the secondary symptoms are so obscure that it is difficult to get any definite information. Of course we must have something definite to go by if we are to establish a standard method of treatment for syphilitic patients, but it is a question whether we should depend upon the specificity of the Wassermann test since it has been found that many other conditions give a positive Wassermann reaction,—conditions having no relation to syphilis.

Dr. Beck truly asks "Is it fair to subject one of these women to rigid antisyphilitic treatment who shows no evidence of syphilis but a positive Wassermann reaction?" Of course it is true that we may not be able to find such evidence until the child is older, nevertheless, I think we should exercise great caution in subjecting women to the prolonged treatment that this disease requires. I should like to ask whether the diagnosis in infants stillborn is based upon autopsy findings or merely upon the fact that they were stillborn. In determining the presence of syphilis we have made use of a history of miscarriages and still-births. If a woman gives a history of one or two stillborn children, we say that she probably has syphilis, and that the stillbirths are due to this cause. In this we are liable to err. In many of these women the miscarriages are due to external causes; they may be induced and we attribute them to syphilis, so we have to be extremely careful in diagnosing syphilis from this source alone.

DR. FREDERICK W. RICE.—At Bellevue and Manhattan Maternity Hospital we have been taking the Wassermann reaction on every pregnant woman and we have been giving vigorous treatment. If we find a positive Wassermann reaction in the prenatal clinic we turn them over to the genitourinary department for salvarsan treatment every two weeks with mercury between. The babies we treat for one month and then turn them over to the children's clinic. Just what the results will be I do not know as we have not made any study of our statistics.

The Wassermann test has also been of great value in allowing us to give the babies who are in need of more nourishment to mothers to nurse who are free from syphilis.

DR. BAILEY.—I should like to ask a little more about the dangers of giving salvarsan. One case arriving at the hospital three days after its use presented the symptoms of acute yellow atrophy and this patient died. The medical examiner refused to do an autopsy because he said deaths were frequent from this cause.

DR. BECK.—I was able to follow up in all, 73 cases in which the Wassermann reaction was four-plus in this series. We reported only 32 cases because of doubt due to the insufficiency of the follow-up records. Many more cases will be included in the complete reports. However, I believe I have included all the deaths. In the beginning of the study the question came up, "did all these women have syphilis who gave a positive Wassermann reaction?" Up to eighteen months ago I refused to treat a woman when she had a four-plus Wassermann and no other evidence of syphilis. The majority of the women gave negative histories of syphilis, and did not recollect having had a rash or a sore, as Dr. Kosmak has pointed out is usually the case. Hence one must rely upon the Wassermann reaction, and because of the severity of the treatment I have insisted upon having each Wassermann checked up by a second test, either in my own or in some other laboratory. Twenty-nine of these 32 women were definitely syphilitie. Three of the cases had single four-plus Wassermann reactions and showed evidences of syphilis.

From what I can gather from the literature and from our own observations, I think it is the consensus of opinion that whether the Wassermann reaction is four-plus or negative during the first month of life does not signify much. Infants may show a positive reaction and then a negative one and never show any evidences of syphilis. I think we do not know all that we should about the Wassermann reaction in pregnancy and in the newborn.

None of these cases were autopsied. One occurred in Bellevue Hospital, and I could not say that the death was not caused by syphilis as I did not wish to place the benefit of the doubt on the good result side. However, I cannot say that these children died of syphilis. One death occurred in the outpatient series and two miscarriages occurred at home, in which I did not have the opportunity to do an autopsy. We based our diagnosis of syphilis on the positive Wassermann and other evidence of syphilis in the patient.

With regard to Dr. Bailey's remarks, we do not hesitate to give salvarsan, and we give it in a large number of patients. These 32 cases were merely picked ones. We have a great number of cases in which one, two or three injections of salvarsan have been given, that are not included in this series. Hirst has said that he is afraid to give salvarsan in the last two months of pregnancy, but most of the cases in this series have been given salvarsan in the last two months of pregnancy without bad results. They were treated in the genitourinary department and the pediatric clinic. We have used different preparations and have not found any that gave severe reactions. We have given it in several cases in which there was nephritis and toxemia and they were not harmed. The blood pressure and temperature were watched. There was no rash antepartum or other bad results.

Department of Reviews and Abstracts

CONDUCTED BY HUGO EHRENFEST, M.D., ASSOCIATE EDITOR

Collective Review

Antenatal Syphilis

By A. N. Creadick, M.D., New Haven, Connecticut

INCIDENCE

TWO means are available for combating syphilis; first, by isolating Leach patient during infectious periods, and secondly, by controlling hereditary transmission of the disease. The latter problem is forced upon the obstetrician and gynecologist particularly by reason of the fetal morbidity and mortality. History of previous infection and evidences of the disease are so difficult to elicit from female patients1, 2 that routine complement-fixation tests on each case are necessary as well as histologic examinations of the placenta and necropsies on fetal and infant dead before syphilis can be excluded. The fundament of a preventive campaign is first a knowledge of who is diseased and how many are diseased. The estimates of the prevalence of syphilis in the population as a whole are not in agreement and none can stand the test of examination. Morrow for the United States, Fournier in France and Blasko in Germany have been quoted for years but the time is certainly ripe for a verification or contradiction of these figures. Two methods are available for the achievement of accurate statistics, (a) making syphilis compulsorily reportable and (b) analyzing statistics from a cross-section of the population and correcting for the whole. A third method suggested by Malzberg³ is a routine Wassermann on applicants for insurance. The investigation of the incidence of syphilis in the population is attended by some difficulty, for instance, "the relation of syphilis to stillbirth and infant mortality is such that we feel morally certain the effect is severe; yet the actual figures are more difficult to secure, for the question of failure to report stillbirths and pardonable zeal to suppress the real cause, when that is syphilis, is understood." Besides, a large amount of recent syphilis is treated by quacks who certainly do not report their cases. A third fallacy in connection with mortality statistics lies in the fact that a large group of syphilities are reported as dead from secondary diseases without the syphilis being noted.

The most reliable cross-section of the population may be found in general hospital admissions. However, in using such figures allowance must be made for the fact that this is a special group, and that it contains a higher proportion of poor and well-to-do, with a lesser proportion of the middle class. Perhaps one of the fairest observations will prove to be those made routinely on pregnant women from all walks

of life applying to well-organized woman's clinics for care. In 4000 such cases reported by Williams,⁵ 2.48 per cent of white and 16.29 per cent of negro patients were syphilitic. At Ann Arbor, Reuben Peterson⁶ collected 28 cases of syphilis from 477 consecutive admissions or 5.8 per cent. This author estimates the number of syphilities in the United States as between 3,000,000 and 4,000,000. From the Sloane Clinic¹ out of a series of 1320 admissions 8.7 per cent were positive by the Wassermann test. Other smaller series have been published whose findings vary more widely as one would expect.^{7,8}

Adair9 collected statistical evidence showing that syphilis plays an insignificant rôle during the first two trimesters of pregnancy. While Routh¹⁰ believes the lesion is inactivated during this period by fetal or chorionic "resistance ferments" analogous to the proteolytic enzymes associated with implantation, all clinicians agree that the serious effects as regards fetal life manifest themselves after the seventh lunar month and increase rapidly during the remainder of pregnancy and labor. For the mother it is claimed that puerperal morbidity, nephritis in pregnancy, and sepsis are more common complications in luetic patients.11 Certainly for the infant an astounding morbidity and mortality can be demonstrated. "Congenital syphilis is more serious than acquired" and is more difficult to eradicate. 12 In Tarnier's Clinic between January 1, 1915, and August 31, 1916, there were 118 pregnancies in syphilitic patients cared for, of these 60 per cent ended in stillbirths and 40 babies or 22 per cent were taken home alive.¹³ Mott collected histories of 175 pregnancies in 34 syphilitic women which showed 104 infant deaths, premature and stillbirths, with 41 living diseased children and 30 healthy children. Mr. Bishop Harman, for a comparative study, collected the histories of pregnancy in 150 syphilitic families and in 150 apparently nonsyphilitic, rating them in terms of 1000:

FAMILIES	PREGNANCIES	STILLBIRTHS AND MISCARRIAGES	INFANT DEATHS	ALIVE BUT DISEASED	HEALTHY
150 syphilities	1001	172	229	390	210
150 nonsyphilities	1000	94	114		792

Hornung¹⁴ cites 221 cases of syphilis in pregnant women collected in Kiel between the years 1910-1914, 13 per cent of these pregnancies terminated prematurely (one-third due to other causes than syphilis) and 30 (13.76 per cent) resulted in stillbirth after the fourth month, therefore 85.33 per cent of these pregnancies resulted in living children. Because of the contrast to the English figures given above and other German figures about to be quoted, mention must be made of the fact that some of these patients received antisyphilitic treatment. Kaufmann-Wolf and Abrahamson¹⁵ have finished the fifth of their series, the totals of which show that 25 per cent of the children in 134 infected women grew up healthy, 25 per cent were diseased, and 50 per cent were dead of syphilis.

Obviously, antenatal syphilis is not only responsible for a large percentage of stillbirths and infant deaths, but also is far-reaching in childhood, adolescence and early adult life (vide infra "Tardy hereditary syphilis"). This is no less striking in American statistics as com-

piled by Royster¹⁶ who found 12.5 per cent of 1000 children who had inherited syphilis applying to a free clinic for treatment; of the white children 7.04 per cent and of the negroes 15.47 per cent were affected. Churchill and Austin¹⁷ give the incidence of hereditary syphilis in New York, Chicago, St. Louis, and San Francisco hospital infants as varying from 2 to 6 per cent of the total admissions. Based on such reports Kolmer 18 pleads for further study and prevention of prenatal syphilis. Sabouraud19 called attention to the fact that it used to be said of Fournier that "he wanted to make of general pathology an annex of syphilography" and that time has but proved that he did not go far enough. Sabouraud recites problems which require further study such as the question of the subsidence of the disease with age, the curve of the Wassermann reaction in hereditary syphilis over a long period of years and the relation of the gravity of acquired as compared with congenital syphilis. Persevering, systematic research will solve these problems as well as the question of conceptional syphilis and its effective treatment.

PATHOLOGY

Indubitable evidence for the diagnosis of syphilis is based on the demonstration of the Spirochete pallida, for which is claimed a genitotropic tendency.20, 21 This organism is found in the semen of syphilitic males independent of evidence of a specific orchitis. 22, 23 The spermatozoa in these cases show numerous abnormal forms, 55.8 per thousand as compared with 18.6 per thousand in the normal healthy male. Incidentally this pathology of the spermatozoa is considerably greater in cases of inherited syphilis at puberty.24 Evidence of the typical external primary lesion in the female is so seldom procured that another avenue of entrance for the organism must be available. It has been suggested that the uterine cavity, tubes and peritoneal cavity are a possible avenue of entrance for the semen-borne spirochetes. Granular, spore-like, resting, nonflagellate forms of spirochete have been described 25, 26 and may be present with the spermatozoa. Clinical manifestations of congenital syphilis differ in type, in malignancy, and result, depending on the existence in one or both parents of latent lues of long standing, conceptional infection or postconceptional infection. Brown and Pearce²⁷ in experimental syphilis in rabbits claim that the reaction of pregnant or lactating females when inoculated is ordinarily most resistant, while occasionally this resistance is absent and a florid invasion occurs. The specific infection of one or both parties to conjugation results in syphilis in the offspring, demonstrable by continued and systematic examination and serological tests. Very rarely apparent exceptions to this rule may be found when the disease in the parent is of long-standing, latent and of low virulence.

Manifestations of the disease during gestation may be divided into changes (a) in placenta and membranes and (b) in the fetus itself. The specific organism may be found in the placenta from about one-third of the cases ^{21, 28, 29, 30, 31} although only after a very careful and prolonged search.^{32, 33} The histologic picture of placental syphilis, first described by Fraenkel,³⁴ has been questioned in detail especially by Hitschmann and Volk,³⁵ but is maintained^{36, 37} as specific when found and more reliable than the serological tests. Unfortunately, syphilis of the fetus with death in utero may take place without the

typical placental histology and without a demonstration of the treponema in the placenta, on the other hand the organism is much more frequently demonstrated in the umbilical cord.^{38, 39} Histologic evidence of syphilis of the cord was described by Bondi⁴⁰ as a polymorphonuclear leucocytic invasion of the perivascular spaces. The specificity of this picture for syphilis has been denied.⁴¹

In the fetus spirochetes are more readily found in the liver,42 spleen,43 kidney and, in addition, typical histologic pictures are apparent in other organs.44, 45 The clinical course and physical signs of congenital syphilis in children born alive is reviewed by Post⁴⁶ who emphasizes the well-known stigmata, snuffles, rhagades, cranial exostosis and defective development. A complete bibliography of this subject has been compiled by Gralka. The most striking picture is presented by the facies and the typical skin picture. Apparently the skin lesion may be bullous according to Sequeira48 and Stelwagon49 when presented at birth and later becomes maculopapular. By the time the patients were one month old or more, Veeder and Jeans⁵⁰ in a study of 100 cases reported the presence of an exfoliative, desquamative dermatitis in 50 per cent and no instance of the bullous type. Highman⁵¹ believes that the type of skin lesion indicates the stage of development of the fetus in utero at the time of its infection. Carman⁵² has collected the literature and described bone lesions pathognomonic of syphilis and Shipley with his collaborators⁵³ studied the skeletons of 300 white fetuses from the sixth month of intrauterine life to term in which they demonstrated typical syphilitic lesions marked in 25 per cent and well marked or suspicious in 46 per cent. All the bones are involved but there is apparently no interference with skeletal growth.54

When gross lesions of an active process are lacking in the mother, before demonstration of the organism in the fetus or the histologic pathology in the placenta is possible, the uniform routine application of the complement-fixation test will demonstrate syphilis. 57, 58, 59 There are qualifications of this broad statement, for occasionally faintly positive reactions appear in the nonsyphilitic, due to a too delicate cholesterinized antigen or some intercurrent metabolic disease like eclampsia. 60 However, there is no clear proof that syphilis may not underlie this last type of case for there seems to be no relation between the degree of toxemia and the strength of the reaction. 57, 60 Nor does the fact that the reaction disappears have a definite bearing for Menten⁶¹ claims that pregnancy arouses a latent syphilis, affording a positive Wassermann reaction which eventually disappears after delivery. The converse of the qualifying statements would be of even more clinical importance but the instances are few in which the complementfixation tests are negative and in which ultimately syphilis is demonstrable by other means. The complement-fixation test on the newborn is very much less reliable. 62 Tests on fetal blood from the cord are anticomplementary, and positive reactions are obtained less frequently until the infant is one month old. After that age until ten or twelve years pediatrists regard the test as specific. Ross and Wright⁶³ checked their series by different means. Again it appears that positive findings are more valuable than negative. DeBuys⁶⁴ alone and with Loeber⁶⁵ correlated the luctin intracuticular test and the Wassermann tests. His conclusions are that the luctin test is more specific

in early infancy, is available when blood for the Wassermann cannot be secured, but when both are negative, syphilis cannot be excluded in all cases. Gordon's results⁶⁶ corroborate these findings. Two series of cases with cerebrospinal involvement have led Jeans⁶⁷ to claim that one-third of the children with congenital syphilis develop such lesions and Kingery⁶⁸ reported lumbar punctures which give specific findings before the paralytic lesions occur.

TARDY MANIFESTATIONS OF INHERITED SYPHILIS

The unusual and later manifestations of the disease may be of more interest to the pediatrist and internist, but serve, in such a review as this, to impress the profession with the necessity of adequate antenatal treatment of the mother and intensive care of the newborn even in the absence of gross evidence of the infection. In one instance 69 an ulcer which appeared during the first month at the site of the umbilical stump and resisted ordinary measures, gave a positive smear and healed under specific treatment. A larger and less well-defined group represent what Zerbino70 refers to as the "distrofia heredosifilitica" consisting of malnutrition, underdevelopment and imperfect digestion, ascribed by Ribadeau-Dumas⁷¹ to affection of the liver, pancreas and parotid. These cases, according to Hutinel^{72, 73} are less resistant to intercurrent infections and may account for nephritis, rachitis and certain of the anemias in a second generation. The lastmentioned author and others74, 75, 76 make more of the frequency of involvement of the liver, spleen, brain and glands in congenital syphilis. "Pluriglandular dysfunction" is noted because of the coincidence of syphilis and hydrocephalus, 50 in 54 cases, in 59 of 69 cases of hypothyroidism, in 34 of 46 cases of mongolism and in 15 of 17 other endocrinopathies.77, 78 Single instances are reported of splenomegaly,79 vitiligo,80 osteogenesis imperfecta,81 ascites,82 "Dysenteroid Syndrome''83 and Vaquez' erythemia84 associated with inherited syphilis. 85, 86, 87

Much less vague and more directly attributable to the specific infection are the cerebrospinal lesions, fulminating deafness, so sudden blindness, backwardness and imbecility. Lavergne goes so far as to require routine lumbar punctures before acquitting a case of suspected lues.

The subject of dental stigmata in heredo-syphilis has provoked some controversy. Stein⁹² emphasized the fact that such lesions (a) may be the only evidence of the disease; (b) may call attention to the etiology of some other baffling condition; (c) may be of assistance in diagnosing a disease affecting some member of the family, as mother, brother or sister; (d) is of importance in life insurance examinations and (e) will aid in enforcing dental, oral, social, moral and mental hygiene. The particular lesions are (a) Hutchinson's teeth,⁹³ (b) cuspal erosions of 1st permanent molar, and (c) multiple and systematic dystrophies of the permanent teeth. These lesions are due to a pathological process of long duration, (a) that of the first molars occurs in the sixth month of intrauterine life and (b) that of the central incisors and canines at the time of birth.⁹⁴ Sabouraud added a specificity to the lesion of the first molar, the so-called "tubercle Carabelli" but such specificity has been denied.⁹⁶ Frequently the first of the tardy syphilitic lesions

are those discovered by ophthalmological examinations. Motor defects and keratitis97, 98 occur most frequently.

COLLES' LAW

In 1837 Abraham Colles promulgated a dictum to the effect that a syphilitic father might engender a syphilitic child without the appearance of the disease in the mother who remained immune to the infection. Matzenauer in 1903 first attempted to discredit this "law" and following the discovery of the spirocheta, 1903, and the Wassermann reaction, 1907, more and more doubt has been voiced until all but a few authors deny its tenability.

Presumptive evidence, originally solely clinical, for Colles' law was based on the fact that so few mothers of syphilitic children themselves gave a history or evidence of active syphilis; even conceptional syphilis, the choc-en-retour of Fournier, seldom resulted in an active process in the mother. For the specific rendering of the law an infection of the embryo of paternal origin is requisite exclusive of the maternal pathway, and the subsequent development of an immunity on the part of the mother. This might be effected (a) by erosion, (b) by the inoculation of the ovum, or (c) by the spore-like granules.12 Colles' law is discredited on the basis that the spirocheta is too large to be carried on or in the spermatozoon.21 Besides, so many Wassermann tests demonstrate a positive infection in mothers otherwise apparently free from the disease that Fildes voices the general opinion in saying that all the mothers are infected. Until scientific investigation can demonstrate the avenue of paternal infection and the decidual barrier to prevent maternal infection this position is the stronger. On the other hand, Hochsinger, 100 Boas, 101 Routh, 10, 12, 25 and Williams, 102 maintain a more conservative stand and do not admit of the verdict "disproven" for Colles' law, and prefer to "hold the whole question sub judice." This position is supported by the occasional instance of syphilis in one of twins and in those few cases where syphilis in the mother cannot be demonstrated while the disease is apparent in the father and child. Even a positive maternal Wassermann during pregnancy may occasionally be regarded as a manifestation of the establishment of active immunity on the part of the mother against a syphilitic product of conception. 103, 104

Singer¹⁰⁵ reported a case of syphilis in one of twins and collects twelve similar cases from the literature, although none have been so completely followed as that cited by Williams. In view of the fact that a positive Wassermann in the expectant mother indicates active antisyphilitic treatment in order to guarantee a living child, the particular means of infection must await further study.^{106, 107, 108}

TREATMENT

Intensive antisyphilitic treatment of the expectant mother is urged on the practitioner owing to the gravity and incidence of the disease¹⁰⁹ in the hope of developing either a diminution in virulence or an increased resistance.^{110, 111,112} The spirochaeticidal qualities of the treatment and the development of antibodies may proceed through the placenta or through the milk. This explanation was offered by Ehrlich for those cases in which the infant improved while the mother nursed it and was receiving salvarsan. The institution of treatment

before pregnancy justifies a favorable prognosis for the fetus. 113 During pregnancy the prognosis depends on the already present but unknown degree of the infection of the fetus. For the adequate control of the treatment of the mother compulsory notification with emphasis on the special need in that case is advocated by Findlay.114 The beneficial effects of partial and adequate treatment of the mother as compared with the neglected cases is graphically charted by Williams. If the mother's Wassermann can be brought to doubtful or negative, the baby is negative. 115 In addition to this treatment all infants should be subjected to further antiluetic treatment after birth. 50, 116, 117 The detailed care of the newborn infant with hereditary syphilis is explained by Jeans¹¹⁸ in detail in reporting a series of 250 cases. This treatment consists in .03 gms. of mercuric chloride per kilogram of body weight intramuscularly weekly and 13 mg. of mercury in chalk by mouth t.i.d. Every two months .01 gms. arsphenamine per kilogram of body weight preceded by a spinal puncture to avoid a Herxheimer reaction. Ramsey and Groebner prefer the 50 per cent mercury ointment and Blechmann prefers 0.02 gms, per kilogram of neoarsphenamine every six or seven days while Fordyce120 gives larger doses weekly for six to eight weeks.

Pinard's¹²¹ and Villapadierna's¹²² cases of postconceptional syphilis prove that there is no feticidal action of salvarsan, neither is there any abortifacient action. Two deaths have been reported, one from acute atrophy of the liver¹²³ in a syphilitic woman three months' pregnant and the suspected cause was salvarsan. In the other case eclampsia and coma supervened upon an injection of "novarsenobenzol" emphasizing the fact that the arsenical preparations are contraindicated in the toxemias and in nephritic insufficiency.¹²⁴ Moore reports the cure of a child with congenital syphilis by means of Dono-

van's solution.125

SOCIAL PROBLEM

"The rising tide of heredosyphilities" with the resultant economic burden 127 on the community emphasizes the need for segregation during treatment 128 or at least intensive cooperation of the syphilologist, obstetrician and pediatrist in providing thorough study and care of each patient. 129

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Selected Abstracts

Eclampsia

Young and Miller: The Etiology of Eclampsia and the pre-Eclamptic State. British Medical Journal, April 2, 1921, No. 3144, p. 486.

This paper is published as a confirmation of one issued in 1914. In the first paper the authors were concerned chiefly with the study of the pathology coupled with some experimental work. In this paper they take up the clinical aspects. The proof is derived from the study of some forty cases of toxemia and associated conditions. The following conclusions are set forth in their summary: (1) These conditions are caused by placental degeneration due to interference with its

blood supply. (2) In these cases placental infarction is due to thrombosis. (3) Absorption of placental poisons only occurs through portions of the placenta attached to the uterine wall. (4) Toxemia may be associated with placenta previa or premature detachment of the placenta. (5) Toxemia develops after the hemorrhage and is not to be considered the cause of the bleeding. (6) The major symptoms of eclampsia are due to the absorption of broken down liver cells, and possibly of other tissues, which are killed by the placental poison. (7) The authors compare this with recent investigations on shock (Quenu, Delbet, etc.) showing that the disintegration products of the muscle are toxic.

F. L. Adair.

Royal Society of Medicine: Toxemias of Pregnancy. British Medical Journal, 1920, No. 3130, p. 976.

At a recent meeting of the Section of Obstetries and Gynecology of the Royal Society of Medicine, toxemias of pregnancy were discussed. Clifford White read a paper on sodium bicarbonate tolerance in toxemias of pregnancy. In these cases sodium bicarbonate was administered in dram doses until the urine became alkaline to litmus. If more than 2 drams (8 grams) were required it was concluded that some acidosis existed. He found that severe cases of eclampsia might require as much as 20 grams before the urine became alkaline.

James Young and Miller presented a paper elaborating and confirming the theory of infarction and disintegration of portions of the placenta as a causative factor in eclampsia. It was suggested that some of the major symptoms were due to the effect of the poison on liver cells, leading to the absorption of broken down products. It was thought that postpartum seizures might be accounted for in this way. Those discussing the paper were not willing to accept Young's theory in the total.

Major C. A. F. Hingston presented a paper emphasizing the necessity of the reduction of blood pressure in eclampsia. He regards it as a serious complication and advocates that it be kept down to 120 mm. or less. He considers venesection to be the best means of reducing the blood pressure. Immediate delivery is also advocated in cases where it is possible.

F. L. Adair.

Schönfeld: The Toxicity of Placental Lipoids and Its Rôle in the Etiology of Puerperal Eclampsia. Deutsche Medizinische Wochenschrift, 1921, xlvii, 270.

By means of various solvents Schönfeld extracted a fatty mass from fresh human placenta. From this mass he isolated a substance which when injected into animals, produced the same symptoms and microscopic changes of the parenchymatous organs as are found in puerperal eclampsia. This substance, which he is inclined to consider a ferment, is activated by the addition of glycerine and inactivated by heat (75° C.).

Small doses of the substance, repeatedly injected, caused a congestion of the pelvic veins and in pregnant female mice it usually produced abortion. In those cases where abortion did not take place before death, hemorrhages were found in the membranes. Even in those cases where the animals died within from 5 to 10 min. after injection, changes were noted in the liver cells.

R. E. Wobus.

Gessner: Why Are the Placental Theories of Eclampsia, Especially Hofbauer's Ideas of Placental Ferment Intoxication, Untenable? Zentralblatt für Gynäkologie, 1921, xiii, 469.

Observation of cases of eclampsia, associated with hydatid mole or without fetus made possible Veit's placental theory of eclampsia, a

poisoning with syncytiotoxin.

Further investigation proved that the placenta had nothing of the character of an antigen and this theory fell to the ground. Hofbauer thought the condition resembled anaphylactic shock, but there was no similarity between these conditions, since lower temperature, lower blood pressure, and lessened coagulability of the blood are absent in eclampsia. Today Hofbauer has given up this idea, but still believes in an intoxication as the result of a poison from the placenta, a so-called ferment intoxication. This theory, based on an altogether wrong idea, i.e., that the human race is the only one in which eclampsia is present, falls to the ground when it is realized that many other animals with similar placental circulation do not have diseases similar to the eclampsia of the human, but on the other hand, animals with different placentation have diseases similar to eclampsia.

In certain cases large quantities of placental ferment may reach the maternal blood without damage to the maternal liver or kidneys. For example, in chronic nephritis, even if there is a marked infarction of the placenta and a sudden discharge of ferments into the maternal blood, eclampsia is comparatively rare. If a nephritic, or a patient with damaged liver can overcome a sudden discharge of such ferment into her circulation, it would appear reasonable that a healthy individual would be affected even less.

W. M. LITTLE.

Zweifel: The Fetal and Placental Theories of Eclampsia. Zeitschrift für Immunitaetsforschung und Experimentalle Therapie, 1921, xxxi, 22.

After a thorough consideration of all the various theories so far advanced to explain eclampsia, the writer takes up in particular the idea of Weichardt that eclampsia represents an anaphylactic shock. Much literature has been published to repudiate this theory, and a series of careful experiments, carried out by Zweifel, force him also to the definite conclusion that this is merely a speculative hypothesis. The injection of fetal and placental albumer from one animal into another of the same species does not cause that hypersensitiveness which would be essential for the development of an anaphylactic shock.

Hugo Ehrenfest.

Van Cauwenberghe: Tuberculosis and Eclampsia. Gynécologie et Obstétrique, 1921, iii, 428.

The coexistence of tuberculosis and eclampsia during pregnancy is of such rare occurrence and of such interest that it warrants attention. Three years after the birth of patient's fourth child tuberculosis of both apices was diagnosed. Three months of treatment improved her general condition. Six months after tuberculosis had been diagnosed, she had a miscarriage followed by a curettage. Almost immediately afterwards she again became pregnant and her general condition be-

came worse. The patient was then placed on treatment consisting of, absolute rest, forced feeding and administration of tuberculin. In three months her condition had greatly improved. She gained in weight and at that time there was no albumin in the urine. When the patient entered the hospital the last month of pregnancy, her respiration became difficult, especially at night, and slight traces of albumin appeared in the urine. A week later the dyspnea became more acute, the albumin in the urine mounted and nine days later the patient had a convulsion. She was given bromides and morphine, and bled, removing 300 e.c., followed by injection of one liter of physiologic salt solution. When seen by Cauwenberghe, patient was in a profound coma. Cervix was dilated and version and extraction performed. Rapid amelioration of symptoms after delivery. Patient became conscious. The following day, however, dyspnea supervened which finally led to an asphyxia.

It seems little likely that the tuberculous toxin or tuberculin exert a direct influence, although Sergent has stated that tuberculosis often produces renal insufficiency. The possible indirect action of the forced feeding should be considered. In the causation of eclampsia the author holds to the intestinal origin of the responsible toxins. Everything that changes the proper functioning of the intestinal tract in pregnancy prepares the way for eclampsia. Thus forced feeding, as is practiced in the treatment of tuberculosis may, under certain conditions, lead to this result.

A case of combined tuberculosis and eclampsia must be considered from the standpoint of the two forms of treatment advocated both in the treatment of pregnancy complicated by tuberculosis, and pregnancy complicated by eclampsia, namely, operative intervention or conservatism. It seems that the coexistence of these two affections in the same pregnant woman forces one to lean toward intervention which is the form of treatment advocated by the author.

Van Cauwenberghe is of the opinion that in cases of tuberculosis one should interfere in the early part of pregnancy. Toward the middle of pregnancy he prefers to wait until the infant is viable. In the seventh and eighth months he believes in interference only to save the woman from the fatigue of the last weeks of pregnancy, though realizing that in spite of the intervention an aggravation of the tuberculosis will probably take place, though to a lesser degree than after dedelivery at term. In tuberculosis of the larynx one should always interfere immediately.

In eclampsia the author advocates interference in addition to medical treatment. The method of interference will depend, of course, upon whether the patient is a primipara or a multipara and whether the eclampsia begins before labor, at the beginning of labor, or near the end of labor.

In the case under discussion, outside of the general indications for intervention offered by the eclampsia, the added imminence of asphyxia as the result of the tuberculosis seemed to demand interference.

The author wishes to draw attention to the fact that tuberculosis places a pregnant woman in a state of lessened resistance and can, among other accidents, produce, directly or indirectly, eclampsia and, in the latter instance, can influence the character of this morbid condition.

R. T. LAVAKE.

Zangemeister: Puerpural Eclampsia. Deutsche medizinische Wochenschrift, 1921, xlvii, 549.

According to Zangemeister, the problem of eclampsia has now been definitely settled. He thinks the solution has been unduly delayed by the *a priori* assumption that the cause must be some form of poison.

This "poison" he finds to be nothing more or less than water.

For some as yet undetermined reason, the blood vessels become abnormally permeable. Water accumulates in the tissues. This first stage is accompanied by oliguria and an abnormal increase in body weight. As the kidneys become damaged, the urine output is diminished, the body weight increases even more rapidly and the blood pressure begins to rise. This constitutes the second stage of the disease. The third stage, represented by cerebral edema, is ushered in by the so-called pre-eclamptic symptoms, such as headache, vomiting and visual disturbances. Upon these follow convulsions and coma.

Proper prophylaxis consists in the timely treatment of the edema by bed rest, and a diet restricting both solids and liquids. In the second stage the diet is still further restricted and salt prohibited. Perspiration may be induced cautiously. Digitalis is contra-indicated and

other diuretics have been found of no value.

In the pre-eclamptic stage the patient is placed in a quiet, dark room and venesection and one or more lumbar punctures made. Morphine or veronal-sodium, of the latter 0.5 gm. intravenously, may be given. Operative interference is always to be kept in mind.

In the treatment of actual eclampsia the following rules have been

established:

1. General: constant supervision, the avoidance of irritation by light, noise, etc.; all necessary manipulations under narcosis; avoidance of food and of infusions.

2. Avoidance of labor pains: if labor is in progress, operative emptying of uterus. Mild pains or after-pains are controlled by morphine.

3. Relief of arterial tension: venesection, except just before or

immediately after delivery; veronal.

4. Relief of intracranial tension: repeated lumbar puncture or

decompression.

In the cases with small, rapid pulse and rapidly rising temperature, mild coma and pallor, heart stimulants and infusions of saline and adrenalin solution are administered. Since these symptoms are due to compression of the medulla, decompression is the only positive remedy.

R. E. Wobus.

Allison: Eclampsia: Further Reports. Texas State Journal of Medicine, 1921, xvi, 444.

Two cases of eclampsia are reported showing certain interesting features. The first case, a married multipara, aged 18 years, had, as estimated by the author, one hundred and fifty convulsions postpartum, followed by complete recovery. This case was interesting further in that lactation did not begin until two months after the birth of the child. At that time she was able to nurse her child successfully.

The second case, a married woman of twenty-six years, had two convulsions before the uterus was emptied of twins, and then several more

convulsions after 91½ hours had elapsed. This patient became totally blind shortly before the onset of the first convulsion. The visual disturbance continued for some time, but complete recovery finally occurred.

NORMAN F. MILLER.

Crainicianu and Popper: Hepatic Insufficiency During Pregnancy. La Presse Médicale, 1921, xliii, 424.

This article involves a study of forty-seven pregnant women during the later months of pregnancy who were selected with an idea of excluding liver affections of any kind. The urine was examined daily for bile salts and pigments and albumin. Blood examinations included many leucocyte counts and numerous observations of the blood pressure. The following conclusions are drawn: In twenty cases out of a hundred there were some signs of biliary retention. In thirty-three out of a hundred, signs were present indicating some latent hepatic affection. The authors report briefly two fatal cases with both clinical and postmortem evidence of dominating hepatic disease. think that it may be possible by proper hygiene, and especially by rational diet, to avoid the causes of this intoxication. They advise against the use of chloroform because of its deleterious effect on the liver, and recommend nitrous oxide and oxygen as the safest anesthesia for these cases. F. L. ADAIR.

Von Jaschke: Kidney Diseases in Pregnancy. Archiv für Gynäkologie, 1921, exiv, No. 2, p. 255.

The kidney of pregnancy as described by Leyden has nothing to do with a purely inflammatory process and the term nephritis gravidarum is therefore to be rejected. The confusion attached to the subject of kidney diseases in pregnancy is increased by imperfections of a nomenclature which has persisted from obsolete conceptions. All processes affecting the kidney, which are of a degenerative nature or of which the inflammatory nature is not beyond doubt, one may with Friedrich Müller group as nephroses, the purely inflammatory form as nephritis. Von Jaschke endorses and reproduces Volhard's scheme of classification of nephropathies into focal nephritis (from infection), genuine nephrosis, sclerosis, and diffuse glomerulonephritis. The nephrosis of pregnancy is characterized by edema, albumin and casts with diminished volume of urine, and an absence of hypertension, cardiac hypertrophy, and eye-ground changes. Histology shows degeneration of the tubules, with no demonstrable changes in the vessels and glomeruli. Rest-nitrogen of the blood is within normal limits and the urineconcentration test (12 hourly specimens while on dry diet) shows no impairment, though excretion of water (8 half-hourly specimens after 11/2 liters of water) is generally delayed. According to Volhard, this reaction to these two tests bespeaks a disturbance in water excretion outside the kidney, namely, failure of the water to reach the kidney because an abnormal permeability of the vessels lets it escape into the tissues. Von Jaschke's limited observations agree with Volhard's dietum that salt excretion falls even to traces in the presence of increasing edema, while in subsiding edema or water equilibrium it is normal or more. Von Jaschke conceives of a toxin which causes the epithelial degeneration and an action on the endothelium resulting in the increased permeability. A milk diet for the kidney of pregnancy is not rational; the diet should be antihydropic. Strict rest in bed is essential. Cases of pure kidney of pregnancy are not frequent, and in prolonged observation of several cases no transition to contracted kidney was seen. Recovery, particularly from the edema, is the rule during the puerperium, though the albumin and casts may persist for months.

There occur, however, cases of pregnancy kidney combined with focal nephritis, giving hematuria, and with diffuse glomerulo-nephritis, giving hematuria and also a blood pressure up to 170. "Probably many of the cases leading to eclampsia, as well as the infrequent cases of pregnancy nephrosis that do not recover during the puerperium, belong to this combination form." It is sometimes possible to elicit a history of chills and fever, but in no case was von Jaschke able to trace definitely the origin of the renal disturbance to an infectious process, and he believes with Volhard that aside from the pure degenerative process with edema and without hypertension "there occurs a form prone to lead to eclampsia, a clinical picture of a typical diffuse glomerulonephritis with a strong nephrotic aspect, which we must designate clinically as a mixed form. But histologically the nature of this nephritis of pregnancy carrying with it hypertension and marked edema is as little explained as is its pathogenesis * * * it may be that the hypertension that is added to the nephrosis depends upon a specific placental toxin and is entirely extrarenal."

As selerosis of the kidney is not common in women of child-bearing

age, it seldom is seen combined with the kidney of pregnancy.

For the further clarification of the still confused subject of diseases of the kidney in pregnancy, von Jaschke urges that all cases be studied with all possible means and observed for long periods afterward.

RAMSAY SPILLMAN.

Vignes: Eclamptic Convulsions. Le Progrès Médical, April 2, 1921, p. 145.

Vignes reviews the recent literature on eclampsia in an effort to form some definite conclusions. As typical, he quotes the statistics of Guillemet of 156 cases observed at the Baudeloque clinic (1890-1914). Of these, 60 occurred before the onset of labor, 67 during labor, and 29 following labor, 108 occurred in primiparae, while only 48 multiparae were affected.

He believes eclampsia to be more prevalent in winter or, at least, at times of sudden drop in temperature, cold bringing on the convul-

sions without much warning.

Among the prodromal symptoms he considers facial edema an especially ominous sign, and the epigastric pain as an indication of imminent convulsions.

Of the 60 women in Guillemet's series who had convulsions before term, 28 went to term under appropriate treatment, and of these, 22 gave birth to living infants. Thirty-two women in the entire series died, giving a mortality of 25 per cent, while 52 of the 156 infants died, or one-third.

As prophylactic measures the following are proposed: All albuminuric patients are put to bed on a strict milk diet. Exposure to cold is to be avoided. Catharties and colonic flushing are used to eliminate poisons. In case of hypertension and excitability, rest, absolute quiet

and chloral are employed, the latter in doses of 4 grams, 10 to 15 gms. per day, given in milk, either by mouth or rectum. This may be alternated with morphine. Venesection is not to be forgotten. Lumbar

puncture has a limited sphere of application.

"Obstetrical treatment reduces itself to nothing or next to nothing." Like his teacher Couvelaire, Vignes feels that labor should be neither induced nor accelerated and one should hesitate to employ forceps or any other method of forcible extraction. He believes that either the classical or vaginal cesarean section, for toxemia alone, are useless since the very grave cases will die anyway and in the more favorable cases the patient can be cured by less drastic measures.

R. E. Wobus.

Bear: Eclampsia and Its Conservative Treatment. Virginia Medical Monthly, 1921, xlvii, 487-491.

After a review of the pathology, etiology, diagnosis and prognosis of eclampsia, the author discusses both the prophylactic and curative treatment of the disease. He does not recommend cesarean section unless the disease is complicated by a contracted pelvis or tumor formation. In his hands best results were obtained by venesection, morphia, catharsis and diaphoresis. Under such treatment the patient usually falls into labor promptly. When fair dilatation is accomplished, a bag is introduced or manual dilatation of the cervix done to hasten delivery.

John W. Harris.

Engelmann: Is the Therapy of the "Middle Line" in Eclampsia Still Justifiable? Zentralblatt für Gynaekologie, 1920, xliv, 1113.

Volhard has drawn a parallel between the pathologic anatomy of the kidney in trench nephritis and in eclampsia. Engelmann believes that a sharp distinction between eclampsia and eclamptic uremia is not always possible; the clinical entity termed eclampsia has no single etiology, but may be produced by different conditions and causes, of which the supposed pregnancy toxemia is one.

It is not logical to omit from the classification of eclampsia those cases in which at autopsy the findings are characteristic, even though there have been no convulsions. The severity of the toxemia bears no relation to the number of these and yet the practical side of the question is that in statistics there is no distinction between light toxemia

with say one convulsion and the very severe toxemias.

A further question is the relation of eclampsia to nephritis. There is eclampsia following nephritis, true eclampsia, and the false eclamptic uremia. In certain clinics the differentiation may be made, but to the majority of practitioners blood chemistry is inaccessible. This is still more complicated by the fact that, according to Volhard, there is a transition from the false to the true uremia, which may occur during pregnancy. Cases that were definitely eclamptic clinically, do not always show characteristic results at section. The recognized anatomical alterations are not specific for eclampsia. The kidney of eclampsia resembles that of Bright's disease. Barr ten years ago declared that the typical alterations in the liver were not pathognomonic for eclampsia.

As true eclampsia should be known that form which we see in pre-

viously healthy primipara, apparently the result of increased metabolism, and that variety that is noted chiefly in multiparae, which apparently arises out of a preliminary nephritis.

Bumm in 1903 declared that immediate delivery was the best treatment. Even in 1911 this was the standard therapy in all teaching influenced by the German school. In 1912 Freund stated that in default of better treatment (not because we are satisfied with the results) we hold to early delivery. This was followed by an article from Leipsic noting remarkable results with the therapy of delay, and one from Dortmund which counseled the therapy of the middle line. Immediate delivery still gave in the best statistics from 3 to 4 per cent of deaths, and was not considered the therapy of choice, particularly in the light of the excellent results obtained in France with conservative measures, especially venesection, Bar and Commandeur noting the renaissance of venesection as the most important development in clinics outside the German influence. Engelmann notes as his therapy venesection with delivery in the usual manner as soon as this is possible without special danger to the patient. In 118 cases he had a maternal mortality of 6.7 per cent, and fetal 9.4 per cent, where the weight was over 2000 grams. In 1917 Freund noted diminished maternal but increased fetal mortality by conservative methods. Two years later Freund again reported that the results had not come up to expectations, but that immediate delivery was not indicated in every eclamptic case. To wait or to deliver must be settled in each individual case. In the meantime Bumm's clinic had given up its earlier stand on immediate delivery, probably on account of unfortunate late results. Unless the cervix was fully dilated, there was an unusually high operative mortality in immediate delivery. Possibly, too, the fetal results were better. Both conservatives and radicals up to a certain extent have given up the extreme position and come toward a more common ground.

The chief objection to conservatism was the high fetal death rate. Engelmann claims that the results will not differ more than about 1 per cent when the middle line is taken. In fact his results have been remarkably improved by transition from active therapy to that of the middle line.

The key word of his position is individualization. Where speed is necessary, abdominal section has distinct advantages over vaginal. Every case of eclampsia exactly as placenta previa should be hospitalized as soon as possible, since each of these complications requires continuous observation and eventually handling which is only possible in a hospital.

Venesection is variable in its effects on the individuual. Results will depend on the stage of the disease when treatment is begun.

Preventives are rest in bed, the minimizing of fluid and of salt, venesection, diructics, chloral, and eventually delivery. Engelmann believes that rupture of the membranes and possibly a forceps delivery is the best method, and believes that the decreased pressure due to emptying the uterus is of the greatest importance. As for chloral, he believes it to be almost specific in treatment, since in addition to its narcotic effect, it possesses the possibility of decreasing temperature and blood pressure, especially when given intravenously.

Contrary to Zangemeister, Engelmann believes in diuretics, particularly purin derivatives. As to infusions there is some difference of

opinion, some holding that, inasmuch as there is retention of fluid in eclampsia, they should not be given. On the other hand, the influence of infusions on diuresis is well known. It is possible that infu-

sions of Ringer's solution may be of value.

Engelmann warns, as does Zangemeister, against forced diaphoresis in all stages of eclampsia. He is doubtful of the worth of decapsulation of the kidney and of lumbar puncture. He draws attention to the known decrease in eclampsia during the years when fat was obtained with difficulty.

H. M. LITTLE.

Brindeau: Decapsulation of the Kidneys in Eclampsia. Gynécologie et Obstétrique, 1921, iii, 275.

The results obtained by renal decapsulation in the treatment of nephritis are sufficiently encouraging to warrant the resumption of the study of this operation in relation to the therapy of puerperal

eclampsia of nephritic origin.

At present the indications seem limited to those cases of eclampsia secondary to nephritis which cause a postpartum anuria, that has resisted all other treatment for twenty-four hours. One would better wait until after delivery before considering decapsulation because the delivery often relieves the kidney congestion.

The operation consists of exposing the kidney by the classical lumbar incision, splitting the fibrous capsule along the convex border of the kidney and decapsulating to the region of the hilum. The wound is closed with a wick drainage. Most surgeons advise decapsulation of both kidneys.

R. T. LAVAKE.

Lübbert: Kidney Decapsulation in Two Cases of Postpartum Eclampsia. Muenchener medizinische Wochenschrift, 1920, lxvii, 1385.

On the theory that failure of the excretory organs to pass off the too rapidly accumulating toxins of pregnancy leads to eclampsia, the kidney becomes the keystone in the treatment of this condition, where convulsions first appear after the uterus has been emptied. Two cases are cited where stripping off the kidney capsule resulted in cessation of convulsions and complete recovery.

Decapsulation in this condition is indicated by the appearance of convulsions after the uterus has been emptied and by an accompanying anuria.

S. B. Solhaug.

Gessner: Increased Blood Pressure in Eclampsia. Zentralblatt für Gynäkologie, 1921, xlv, 847.

The author distinguishes between idiopathic hypertonus in pregnancy based, he believes, on hypersecretion of the adrenals, and possibly due to increased abdominal pressure, and the hypertonus symptomatic of impending eclampsia. He noted a characteristic blood pressure curve in the latter, which he found in only one other condition, namely, mechanical obstruction to urinary secretion. This condition exists in some cases of enlarged prostate and has been artificially produced by injecting fluid into the bladder. That there is a certain amount of back pressure in the urinary tract during pregnancy has been shown by Weibel.

Since similar conditions do not arise from tumors of the pelvis, the

cause cannot be ascribed to direct pressure. In fact, Gessner finds that the ureters are not markedly compressed by the pregnant uterus nor the urethra by the descending head. Since the cervix and neck of the bladder are connected by muscle bundles, the bladder is drawn up by the pregnant uterus. This again pulls on the ureters and causes a spastic condition of the ureteral musculature. If the kidney is movable, no harm is done, but if the latter is fixed by an excessive amount of fat, the spastic ureters, he believes, cause an interference with its blood supply. To this tugging on the kidney, he ascribes the epigastric pain of eclampsia.

The author points out the danger of injecting salt solution after venesection and advises early ligature of the cord in order to reduce the amount of poison in the infant's blood. The latter should be given large amounts of water during the first few days of life. W. M. LITTLE.

Nevermann: The Effect of Venesection in Eclampsia. Zentralblatt für Gynäkologie, 1921, xvii, 609.

It has been claimed that venesection causes a decrease in the total quantity of poison, as well as a decrease in the viscosity of the blood. Nevermann attempts to prove that neither of these theories is tenable. Nor does he believe that the temporary decrease in excessive blood pressure is a factor in the marked improvement following venesection. Observations made during the past year according to Weiss' method with the capillary microscope gave a clue to the manner in which improvement was obtained.

After venesection there was a decided decrease in the tension of the capillaries and increase in the capillary circulation not observed after the administration of any of the medicaments which produce similar results by lowering of blood pressure, etc. The capillary tension did not seem to be of nervous origin, but directly dependent on the amount of blood brought to the capillaries. Decrease in the volume of venous blood naturally released tension and allowed a more free circulation. Venesection has a place in pre-eclamptic toxemia. W. M. LITTLE.

Gibson: The Sequelae and Later Aspect of the Toxic Albuminurias of Pregnancy. Surgery, Gynecology and Obstetrics, 1921, xxxii, 513.

Except in cases of undoubted chronic nephritis, Gibson thinks it futile to attempt a classification of the various forms of disturbances of pregnancy ranging from albuminuria to eclampsia. From a study of eclampsia patients who again became pregnant, he believes that a subsequent pregnancy always exposes a patient to three hazards, namely, irreparable damage to the kidneys, and heart, prematurity, and eclampsia. While a woman may be permitted to become pregnant again after an interval of normal blood pressure and freedom from evidence of kidney insufficiency, he, personally, can see no justification for placing her in jeopardy a second time.

R. E. Wobus.

ERRATA

September, 1921, issue, page 309, in the discussion on Dr. Baer's paper:—Dr. J. C. Howe, should read Dr. Joseph C. Aub.

Correspondence

EDITOR OF AMERICAN JOURNAL OF OBSTETRICS AND GYNECOLOGY, Sir.

The American Society for the Control of Cancer announces a seven days' campaign to be designated "Cancer Week" from October 30 to November 5. The purpose of the movement is to reach as many persons as possible in the United States and Canada, with the vital message of Cancer Control. Committees have been established in all parts of these countries; lecture bureaus, made up of interested and authoritative speakers, have been formed, and the campaign will be conducted in accordance with the enclosed plan.

Naturally the success of even so conservative a campaign depends largely upon the character and extent of the publicity given; while supplementary information from reliable sources by medical writers will be of the greatest value in creating interest and disseminating facts concerning cancer.

There is probably no preventive medicine campaign of more vital importance to all classes of people, or one which gives promise of greater interest or more hope. It is significant that the death rate from this disease has been arrested. Since 1916 it has remained practically stationary, with only minor fluctuations. This is the most encouraging thing which has happened since the Society was organized in 1913, and leads us to believe that united effort by all cooperating agencies will not only prevent a further increase in this rate, but will effect a continuous decrease.

We feel that no small part in the past attack on this insidious disease has been played by writers in medical journals and the lay press; and it is confidently hoped that you will now "carry on" even more determinedly the attempt to rout this, one of the last of the so far uncontrolled scourges.

Faithfully yours,

F. J. OSBORNE, Executive Secretary.

September 2, 1921.